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CONTENTS OF VOL. XXIII

PART 1. JULY, 1932

PAGE

The Life-work of Carveth Read, 1848–1931 1

Tests of Perseveration. I. Their Relation to Character. By J. W. PINARD 5

Some Psychological Experiments on Mental Defectives in Relation to the Perceptual Configurations which may underlie Speech. I. By R. G. GORDON and R. M. NORMAN 20

Intelligence and Fertility. By SHEPHERD DAWSON . . . 42

A New Performance Test of Intelligence. By W. P. ALEXANDER 52

On the Computation of Regression Equations, Partial Correlations, etc. By GODFREY H. THOMSON 64

✓A Gestalt Theory of Humour. By NORMAN R. F. MAIER . 69

Publications Recently Received 75

Proceedings of the British Psychological Society . . . 84

British Psychological Society. Names of Officers and Members *at end*

British Psychological Society. Rules . . . . . . *at end*

PART 2. OCTOBER, 1932

Some Psychological Experiments on Mental Defectives in Relation to the Perceptual Configurations which may underlie Speech. II. By R. G. GORDON and R. M. NORMAN 85

Tests of Perseveration. II. Their Relation to Certain Psychopathic Conditions and to Introversion. By J. W. PINARD 114

The Formation of a Simple Habit in Guinea-Pigs. By G. C. GRINDLEY 127

A Note on the "Binaural Threshold." By F. H. GAGE . . 148

Medico-Psychological Aspects of Normality. By EDWARD GLOVER 152

Synaesthetic Factors in Judging the Voice. By MADELINE KERR and T. H. PEAR 167

The Mathematical and Experimental Evidence for the Existence of a Central Intellective Factor (*g*). By WILLIAM BROWN 171

Visual Imagery and Retention. By HENRY BOWERS . . 180

Publications Recently Received 196

Proceedings of the British Psychological Society . . . 203

PART 3. JANUARY, 1933

PAGE
Victoria Hazlitt, 1887–1932. By A. W. WOLTERS . . . 205
The Peripheral Perception of Movement. By M. D. VERNON 209
Visual Acuity and its Physiological Basis. By WARREN W. WILCOX and DONALD McL. PURDY 233
Rhythmization and Speed of Work. By D. W. HARDING . 262
The Relationship between General Mental Development and Manual Dexterity. By PORTIA HOLMAN . . . 279
Kinaesthetic Factors in the Recall of Musical Experience. By JAMES MAINWARING 284
Temperament Tests. I. Temperament. By RAYMOND B. CATTELL 308
Publications Recently Received 330
Proceedings of the British Psychological Society . . . 339
Balance Sheet 340

PART 4. APRIL, 1933

The Relation of Acts and Contents of Consciousness. By CHARLES S. MYERS 343
A Test of the Theory of Two Factors. By WILLIAM BROWN and WILLIAM STEPHENSON 352
A Critical Discussion of the Single-factor Theory. By J. O. IRWIN 371
The Effect of Delayed Reward on the Maze Performance of Chickens. By A. P. CHATTOCK and G. C. GRINDLEY . 382
A Repetition of Rubin's Figure-ground Experiment. By M. B. DUTTON and P. M. TRAILL 389
A Quantitative View of Pain. By LEWIS F. RICHARDSON . 401
Note on Dr William Brown's Paper on a Central Intellective Factor. By GODFREY H. THOMSON 404
A New Form of Tachistoscope. By G. C. GRINDLEY . . . 405
Publications Recently Received 408
Proceedings of the British Psychological Society . . . 418

LIST OF AUTHORS

PAGE
Alexander, W. P. A New Performance Test of Intelligence 52
Balance Sheet 340
Bowers, Henry. Visual Imagery and Retention 180
British Psychological Society. Names of Officers and Members *at end*
British Psychological Society. Rules . . . . . . *at end*
Brown, William. The Mathematical and Experimental Evidence for the Existence of a Central Intellective Factor (*g*) 171
Brown, William and Stephenson, William. A Test of the Theory of Two Factors 352
Cattell, Raymond B. Temperament Tests. I. Temperament 308
Chattock, A. P. and Grindley, G. C. The Effect of Delayed Reward on the Maze Performance of Chickens . . 382
Dawson, Shepherd. Intelligence and Fertility 42
Dutton, M. B. and Traill, P. M. A Repetition of Rubin's Figure-ground Experiment 389
Gage, F. H. A Note on the "Binaural Threshold" . . 148
Glover, Edward. Medico-Psychological Aspects of Normality 152
Gordon, R. G. and Norman, R. M. Some Psychological Experiments on Mental Defectives in Relation to the Perceptual Configurations which may underlie Speech. I 20
Gordon, R. G. and Norman, R. M. Some Psychological Experiments on Mental Defectives in Relation to the Perceptual Configurations which may underlie Speech. II 85
Grindley, G. C. The Formation of a Simple Habit in Guinea-Pigs 127
Grindley, G. C. A New Form of Tachistoscope 405
Harding, D. W. Rhythmization and Speed of Work . . 262
Holman, Portia. The Relationship between General Mental Development and Manual Dexterity 279
Irwin, J. O. A Critical Discussion of the Single-factor Theory 371
Kerr, Madeline and Pear, T. H. Synaesthetic Factors in Judging the Voice 167
Maier, Norman R. F. A Gestalt Theory of Humour . . 69

PAGE

Mainwaring, James. Kinaesthetic Factors in the Recall of Musical Experience 284

Myers, Charles S. The Relation of Acts and Contents of Consciousness 343

Pinard, J. W. Tests of Perseveration. I. Their Relation to Character 5

Pinard, J. W. Tests of Perseveration. II. Their Relation to Certain Psychopathic Conditions and to Introversion . . 114

Proceedings of the British Psychological Society . . 84, 203, 339, 418

Publications Recently Received 75, 196, 330, 408

Richardson, Lewis F. A Quantitative View of Pain . . . 401

The Life-work of Carveth Read, 1848–1931 1

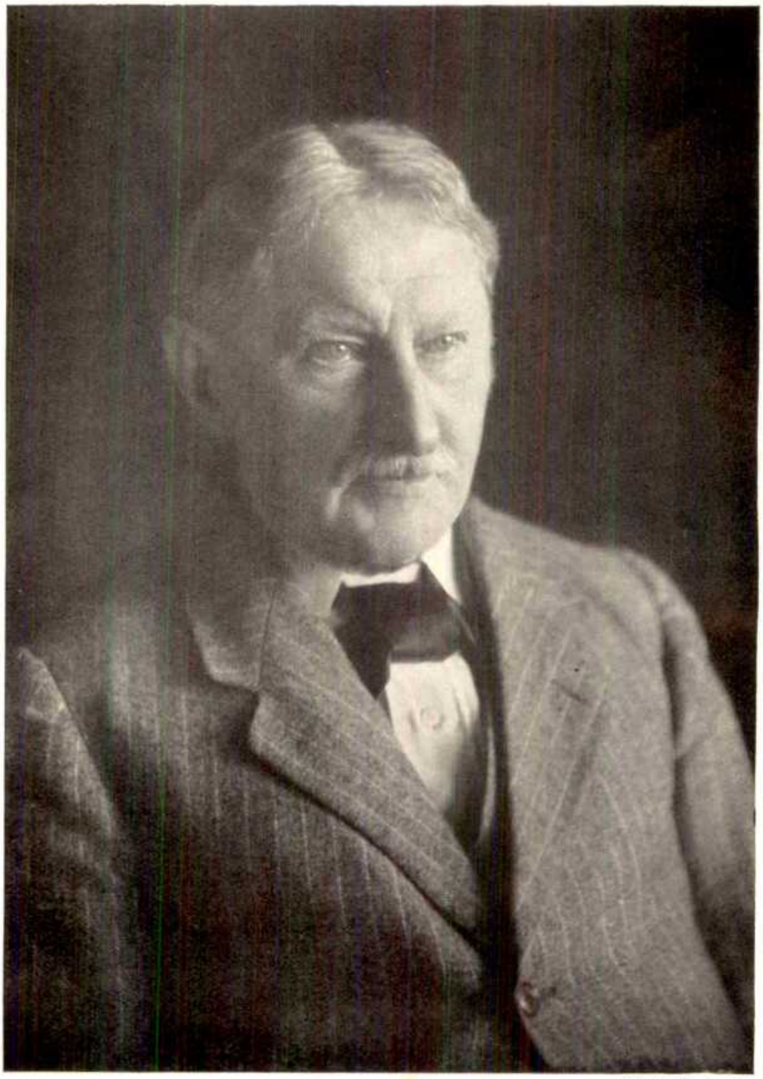
Thomson, Godfrey H. On the Computation of Regression Equations, Partial Correlations, etc. 64

Thomson, Godfrey H. Note on Dr William Brown's Paper on a Central Intellective Factor 404

Vernon, M. D. The Peripheral Perception of Movement . . . 209

Wilcox, Warren W. and Purdy, Donald McL. Visual Acuity and its Physiological Basis 233

Wolters, A. W. Victoria Hazlitt, 1887–1932 205



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PROFESSOR CARVETH READ
1848–1931

VOLUME XXIII JULY, 1932 PART 1

THE LIFE-WORK OF CARVETH READ, 1848–1931.

BORN in 1848, Prof. Carveth Read distinguished himself at the University of Cambridge as a Scholar of Christ's College, and then spent some *Wanderjahre* as Hibbert Travelling Scholar at Leipzig and Heidelberg. His original vocation in life was that of instructor at the famous educational institution of Wren. Here his activities were multifarious; they included at least the teaching of logic, epistemology, ontology, ethics, economics, psychology, and English. Not till 1903 did he turn to academic life, succeeding his great friend James Sully at University College, the University of London, as Grote Professor of Philosophy of Mind and Logic. But this title is somewhat misleading. In truth, Prof. Carveth Read confined himself mostly to psychology, and this non-experimental. But he encouraged experimental work in every possible way, and he did make occasional excursions into philosophy and logic. In 1911, he gave up the Chair, but continued to work at the University as Lecturer in Comparative Psychology up to 1921.

As regards his publications, the earliest topic treated by him—financially, by far the most successful one—was that of logic. After his first venture at a *Theory In Logic* (1878), he brought out twenty years later his *Logic, Deductive and Inductive*, which went through four editions and numerous reprints. Well it might. A perfect elementary text-book in every way, it above all things excelled in that virtue by which the author and others had made the reputation of Wren's establishment; that is to say, it was so written that the reader *had* to understand. Doctrinally, it gave most adherence to J. S. Mill, whilst also showing partiality for Bain, Venn, and Keynes. Other kinships become apparent in his later editions, especially noticeable being some helpful intercourse with Prof. A. Wolf.

Incomparably more ambitious, however, was *The Metaphysics of Nature*, published in 1905 and reaching a second edition in 1908. In this great product, as indicated by the title, we come upon the eventual fruit of the author's work at Wren's in the teaching of philosophy. The book falls into four sections, entitled respectively 'Canonic,' 'Cosmology,' 'Psychology,' and 'The Categories.' Of these the first, second and fourth are so exclusively philosophical that, despite their brilliancy of form and profundity of thought, their discussion does not seem suitable for a journal of psychology. Even the remaining section, though it does deal with psychology, treats this chiefly in its philosophical aspect. Its first chapter is discursive and preparatory; in it the author briefly dismisses "souls" and "the unseen world"; he deals at a little greater length with the "mystery of consciousness," the alleged "substantiality of the Subject," and the relation of this Subject to the body. But his second chapter is vital; it brings him to the following frank profession of fundamental doctrine.

"Neither Materialism nor Spiritualism can give any account of perception or volition. To Materialism all consciousness is a miracle, and therefore so is perception; to Spiritualism no perception is possible, because there is nothing to perceive. Dualism, maintaining the reality both of a material and of a spiritual Substance, has, from Descartes to Hamilton, tried in vain to circumvent the truism that between Substances divided by definition there is no community." "Therefore, I am recommending as the most coherent and natural way of thinking, on the whole, this hypothesis that the World is essentially a conscious thing; that in consciousness we have immediate knowledge of Reality, but not of the whole of Being; that the rest of Being is made known to us by phenomena; that it is everywhere conscious, but in various degrees, and that the higher degrees are known to us by the phenomena of organisation."

The author's next great work followed after the lapse of four years, that is to say, in 1909. It was called *Natural and Social Morals*. Its main interest is not so much psychological as rather ethical and sociological, and so would appear to have its roots not in his present academic activities but still in the older labours at Wren's. In so far as it does enter into the domain of psychology, it is chiefly an anticipation of what follows in a more advanced development later on.

To this later work, then, we may forthwith turn. Here his most important preparatory studies do belong to his university work. They consist in a brilliant series of articles published from 1908 onwards, almost all of them appearing in this *Journal*. All these were co-ordinated, supplemented, and perfected into *The Origin of Man and of His Superstitions*, which appeared in 1920, and had a second edition (two volumes) in 1925. In this, his first care was to develop a theory which had been mentioned by him repeatedly in earlier works, and always with rising emphasis. It was to the effect that Man owed his superiority over the anthropoid stock to his change of diet from vegetables to meat.

"Into his life a disturbing factor entered—the impulse to attack, hunt and eat animals, which extensively replaced his former peaceable, frugivorous habit.... It was advantageous (*a*) in enlarging their resources of nutrition, and (*b*) in enabling them to escape from the tropical forest. ...There was rapid selection of those whose variations of structure, character, activity were most effective in dealing with game and with enemies; especially of those who combined and co-operated, and learned to direct co-operation by some rudimentary speech."

Having thus shown how Man reached the stage of hunting, the work proceeds to consider how he passed on from this stage to the subsequent settled life of the tribe or group. This further advance is attributed to the protective action of Magic and Animism. "These superstitions were useful and (apparently) even necessary in giving to elders enough prestige to preserve tradition and custom when the leader of the hunt was no longer conspicuous in authority." Everything accordingly hinges upon how such beliefs as Magic and Animism could arise and obtain a hold upon all tribes and nations. The answering of this question is the main task of the book.

As always, the author proceeds in admirably rational sequence. First comes a chapter on the nature of belief in general and of its two chief varieties in particular, which he calls respectively "perception-belief" and "play-belief." Here, naturally, yeoman's service is done for him by his older and more philosophical work on Canonics. He finds that the play-belief includes the above-mentioned Magic and Animism. Both of these he discusses at length, concluding in particular that—contrary to the usual opinion—the Magic has been the earlier of the two in human development. In the course of these chapters he has much to tell us about "ghosts" and "souls." Then the book is completed by three chapters on 'Omens,' 'The Mind of the Wizard,' and 'Totemism' respectively.

[Besides all the writings so far mentioned, there were some other contributions to this *Journal* and to *Mind.*]

After thus all too briefly running over the purport of the chief publications of Prof. Carveth Read, a word may be said as to their merits. And to do this summarily about his work in general is greatly facilitated by the fact that this is singularly even. First, as we have already had occasion to notice, he possessed an extraordinary degree of lucidity; in his hands the most difficult matters became transparent. And this applied not only to his writings, but no less so to his lectures. In the second place, his argumentation was exceptionally persuasive; partly because of its logical cogency; and partly because his tone was such as to disarm antagonism. But his reader is more than convinced; he is continually being delighted. For the linguistic style is not merely serviceable; it is beautiful. Not for nothing had he been for so many years teaching how to write the English language. His so fair linguistic tissue, moreover, is dotted over with gems of delicious humour. With such qualities to their credit, we may safely prophesy that his writings will long remain both a profit and a joy in the land.

But there has to be mentioned one more characteristic of his writings, one that in even higher degree than the other was an integral constituent of his personality. This is his selflessness. One manifestation meets us in his scientific sincerity. Even under his earlier conditions of life, which would have turned most men to mere "crammers," he, on the contrary, was all the time, as we have seen, doing such good work that this afterwards served as a solid foundation for his masterpieces. Another manifestation—one of which the present writer can speak from manifold personal experience—was his unceasing kindness. He was always the keenest of men to do good to, or recognize goodness in, anyone else; the least keen to benefit or extol himself; the very pattern of colleagues; the model of teachers; the friend of friends[1].

C. SPEARMAN.

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[1] The preceding account has had the great advantage of kind emendations by four exceptionally well-qualified persons, Professors Dawes Hicks, Wolf, Aveling and Flugel.

TESTS OF PERSEVERATION

I. THEIR RELATION TO CHARACTER[1].

By J. W. PINARD.

(*Research Student of the National Institute of Industrial Psychology.*)

I. *Introductory* (p. 5).
II. *Experimental difficulties* (pp. 6–11).
III. *The tests and methods of scoring employed* (pp. 11–12).
IV. *Perseveration as a group factor* (pp. 12–13).
V. *Sex and age differences* (p. 13).
VI. *Relation to types of character* (pp. 14–18).
VII. *Confirmation by later experiments* (pp. 18–19).
VIII. *Summary* (p. 19).

I. INTRODUCTORY.

A REVIEW of the literature on the subject of perseveration shows that, from the very first use of the term up to the present time, certain types of character have almost invariably been associated with certain views of perseveration. But it was not until recently that any systematic experimental work on the relation between perseveration and character was carried out—by Lankes and Webb (1). Webb submitted forty-seven students of a training college for teachers to estimates of what he called the "persistent qualities" of character, comprising (*a*) perseverance as opposed to changeability, (*b*) perseverance in the face of difficulties, (*c*) kindness on principle, (*d*) trustworthiness and (*e*) conscientiousness. Whereupon Lankes correlated these estimates of character with certain scores for 'perseveration' made by the same students. The correlation was negative and very small at that, thus exposing the fallacy of the belief generally held by psychologists that the strong perseverator was stable in his emotions and steadfast in purpose. In the present investigation it was decided to start work on 'difficult' children, in whom excess or defect in these (and certain other) traits of character would necessarily be more marked and the individual differences greater than was the case for Lankes and Webb's 'normal' subjects.

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[1] The first part of this investigation was carried out under the guidance of Prof. C. Spearman at University College, London; the rest of it was supervised by Dr C. S. Myers and carried out by aid of a grant from the Rockefeller Foundation to the National Institute of Industrial Psychology.

II. EXPERIMENTAL DIFFICULTIES.

Some five hundred children, ranging in ages from 8 to 15, in the Leytonstone Home for children from undesirable homes, were considered the most suitable material available. Permission for research was obtained from the local Board of Guardians and the work started. The matron and staff proved most anxious to help, and a body of psychological investigators under the direction of Dr J. A. Hadfield gave their support and entered willingly into the scheme. Later, the field of inquiry was transferred to the Maudsley Hospital for neuroses and early and recoverable forms of mental disorder, where the medical superintendent, Dr Edward Mapother, and the medical and nursing staffs rendered all possible assistance and were most encouraging in their attitude towards the work done.

It was decided to employ the six tests for perseveration selected by Bernstein (2). But for various reasons which will be set forth below, many alterations had to be made in them when applied both to the children at the Leytonstone Home and to the patients at the Maudsley Hospital.

The majority of Bernstein's tests proved too difficult for these children, of whom some were only 8 years old, others were intellectually below normal, and others again were of an unmanageable disposition. The degree of ease with which these tests can be performed is of the greatest importance, because of their extreme sensitiveness: the least diffidence at the beginning, or the slightest doubt as to how to proceed, again and again proved to influence the final scores so enormously as to justify one in saying that the tests measure doubt or ignorance of their technique as much as they measure perseveration—if not to the exclusion thereof. Consequently very often, after some practice, an apparently strong perseverator turned out to be a weak perseverator, and *vice versa*.

Three tests, selected from Bernstein's battery, were found suitable, viz. "the Inverted S Test" (S.), "the Triangle Test" (△), and "the Mirror Image Test" (M.I.). A fourth test was evolved by the experimenter himself and called the "Alphabet and Number Test" (A.N.). These tests consisted in (i) writing letters and figures for 1½ min. (*e.g.* S S S S S S), (ii) reversing the design for 1½ min. (*e.g.* Ƨ Ƨ Ƨ Ƨ Ƨ Ƨ), and finally (iii) alternating the first with the second for 3 min. (*e.g.* S Ƨ S Ƨ S Ƨ). A fuller description of the tests in their altered and improved form is given later in this paper (pp. 11, 12).

Even the selection of these simpler tests did not altogether overcome the difficulty which the younger, backward and most difficult children, or the most depressed and confused of the adult patients, experienced in performing them. Some of the subjects, many more than one would have supposed, required five practices before they were reasonably confident of the method of procedure. For this reason the actual test was preceded by five practice periods, when the 'backward' subjects were instructed, encouraged and sometimes eliminated. It proved necessary to make every subject go through all these practice periods because of the important effects of practice on the result. A group of twenty-five patients, for instance, averaged the following marks for perseveration[1] for the same tests taken ten times over a period of 11 days: 144, 149, 143, 144, 145, 130, 126, 126, 119, 110. So, too, ten subjects, individually tested thirty times over a period of 5 weeks, gave the following averages for the first 5 days and for the last 5 days respectively:

Table I.

Average for first 5 days	Average for last 5 days
128	21
208	93
74	59
69	55
105	49
274	154
74	40
142	76
105	88
69	8

These are only a few cases taken at random from many groups and from more than seven hundred individuals, which prove that the mark for perseveration often decreases with practice. To ensure uniformity in ranking and grouping, therefore, the same number of practices had to precede the final test for all subjects, irrespective of whether they could carry out the tests after the first practice or not.

Moreover, it was soon discovered that many subjects varied so much in their mark for perseveration from day to day that one would hardly be justified in taking the mark for any one day as the standard of perseveration for any individual. The orders of ranking of ten children, for instance, obtained from the total scores for the four tests which were repeated five times over a period of 5 days, preceded by five practices, were as follows:

---

[1] The method of scoring is described on p. 12. The higher the score, the stronger the perseveration.

Table II.

Ranking on the average of the 5 days' tests	Ranking on each of the 5 days' tests				
	1st day	2nd day	3rd day	4th day	5th day
1	2	2	2	2	2
2	3	3	3	3	3
3	1	1	5	5	3
4	5	5	10	1	1
5	7	4	4	4	8
6	6	6	6	6	6
7	4	9	7	7	9
8	8	8	8	8	8
9	9	7	1	9	5
10	10	10	9	10	10

The ranking of those who in the first column are placed 1, 2, 6, 8 and 10 is practically in agreement with that in the other columns, but those of the remaining five are far from being so. If the girl who ranked 4 in the first column had been tested on the third day only, she would have ranked last and on each of the two succeeding days first. Similarly 9 would have ranked first if tested on the third day only.

Applying the above four tests to 194 children at the Leytonstone Home and correlating the averages of the first and third with those of the second and fourth in the usual way, the following results are obtained:

The reliability coefficient for the first five practice tests = 0·451.

The reliability coefficient for the sixth and seventh tests thereafter = 0·568.

The reliability coefficient for the five tests following five practices (6, 7, 8, 9, 10) = 0·724.

In view, therefore, of the low coefficient of reliability of the tests for the first five applications, it was considered necessary to take the average of the four tests, applied five times over a period of 5 days and preceded by five practices, as a fairly reliable mark for the perseveration of any individual. In this way it was hoped that some standard of perseveration for each subject would be arrived at; and this method of procedure was applied to all the testing and marking that was done after the experimental work on the tests themselves was completed and before any correlation or grouping was done.

Prior to this, however, several other alterations in the tests had to be made. For one thing, the usual 6 min. duration of each test proved too long for both the normal and abnormal subjects of this experiment. The subjects had necessarily to be told to write as quickly as possible, but it was found that many of them, for the greater part of the last minute for each test, suffered from cramp or numbness in the hand which impeded their speed during the latter part of each test. The following experiment was carried out to test the relation between the duration of the tests and the perseveration mark for them. The subjects were told to draw a line at the point they had reached after each minute and the letters and figures for each minute were counted separately, for tests lasting 6 min. and 4 min. respectively. The average figures for 93 girls were as follows, *P* showing the mark for perseveration:

Table III.

	Total output of work	*P*	Output of work for					
			1st min.	2nd min.	3rd min.	4th min.	5th min.	6th min.
For the 6 min. test	732	190	139	157	159	95	99	83
For the 4 min. test	509	99	142	156	—	98	113	—

Note. The third, or 'alternation' section of the test (see p. 6) was introduced after the third and second minutes for the 6 min. and 4 min. tests respectively.

From these figures it is clear that, whereas the practice effect is responsible for a steady increase in the output during the first 3 min. of the 6 min. test, the output decreases from the fifth to the sixth minute by 16; whereas, taking into consideration the practice effect on perseveration, one would have expected an increase. Also, because of the practice effect, one would have expected the total measure for perseveration (*P*) for the 6 min. test to be proportionately smaller than that for the 4 min. test—whereas just the opposite occurs. The effect of fatigue is noticeable at the very beginning of the second half of the 6 min. test. It will be observed that the first 2 min. output for the first and second halves are 296, 298 and 194, 221 for the 6 and 4 min. tests respectively; and that whereas the difference between the first two halves is only 2, that between the second two is 27. Apart from this proof, although no figures were kept, the experimenter could not but notice how many more mistakes were made towards the end of the second half of the 6 min. test than in the middle or even beginning thereof, or in any part of the second half of the 4 min. test. This fact alone naturally raised the question as to how far fatigue, mental or muscular, had an effect on the measurement of perseveration. To minimize the effect of fatigue on perseveration, therefore, the duration of each test was limited to 4 min., and a 2 min. rest was given between successive tests.

Lastly, we come to what must be considered the most important factor in connection with these tests, namely, the presence or absence of a motive for doing them. This point must be specially stressed. To begin with, maximum speed and effort have to be maintained from beginning to end if the perseverative tendency of the mind is to be measured at all. At the Leytonstone Home the experimenter started by applying the tests to groups of 40; but, owing to the difficulty of controlling the children, they were reduced to 30, then to 20, and ultimately to 15, 10 and even to 5 at a time for special cases. This prevented them from carrying out all sorts of tricks, such as turning the papers upside down as in the case of the M.I. test, instead of reversing the movement, or leaving spaces open and inserting afterwards, instead of alternating, as in the A.N. and S. tests.

Even then, however, it was evident to the most superficial observer, that without introducing a strong motive for performing these tests the result could never be a proper measure of perseveration. One had but to study their method of procedure to be convinced of this fact. The most difficult children went about their task mainly in one of two ways.

(1) Certain of them would start off with a will and carry on straight through the first two sections of the test; but, directly the third or 'alternation' section (cf. p. 6) was introduced, they would slacken off considerably and more often than not would carry on only because they were made to do so. These children would get a high mark for perseveration, whereas in reality unwillingness if anything was measured. When by some special inducement such a subject could be made to maintain effort, a very different mark was obtained.

(2) Similarly, another class of child carried on in a leisurely fashion right from the start, so that even when the 'alternation' section of the test was introduced he could maintain the same speed more or less; as a matter of fact, the very fact that a difficulty was introduced at the beginning of the second half seemed to stimulate effort in many cases. These children, then, would appear to be weak perseverators, whereas their score really only indicated initial lack of effort.

The experimenter tried various ways to interest them in the work. On the advice of the staff, who had predicted trouble of this kind, the children were told that the tests were games, but their disappointment was colossal and after this they frankly ridiculed the idea. Then their co-operation was asked for the sake of science; this called forth a little response, but the effect never lasted for more than a few minutes. To stimulate a competitive spirit, the score of the total output of work was posted up from day to day, but many of the more 'difficult' children were not interested. It was not until the writer had taken them out in small parties on the lakes and for rambles in the woods and had joined in their games and sports, when the personal factor was introduced and they tried to please him, that some amount of interest was created and sustained effort and speed were ensured.

At the Maudsley Hospital by far the majority of the patients proved willing, but even here the most depressed of them could hardly become really interested. The upshot of it all is, that notwithstanding everything that was done and said to ensure concentration, the factor of unwillingness could never be eliminated and not more than minimized in its effect on the result as regards the subjects most concerned, namely, the 'difficult' ones. And sensitive as the tests are, this fact must be considered of the greatest importance in interpreting all groupings and rankings.

III. THE TESTS AND METHODS OF SCORING EMPLOYED.

1. *Inverted* S *test.*

(*a*) The subjects were instructed to write "S S S S S S ..." as fast as possible for 30 sec.

(*b*) The subjects were told to write the reversed letter "Ƨ Ƨ Ƨ Ƨ Ƨ Ƨ ..." as fast as possible for 30 sec.

(*c*) (*a*) and (*b*) were then repeated.

(*d*) The subjects were finally told to write "S Ƨ S Ƨ S Ƨ ..." for 2 min.

The full procedure was therefore as follows:

(1)	S S S S S S ...	30 sec.
(2)	Ƨ Ƨ Ƨ Ƨ Ƨ Ƨ ...	30 sec.
(3)	S S S S S S ...	30 sec.
(4)	Ƨ Ƨ Ƨ Ƨ Ƨ Ƨ ...	30 sec.
(5)	S Ƨ S Ƨ S Ƨ ...	2 min.

2. *Triangle test.*

(*a*) The subjects were instructed to draw a series of triangles—apex upwards—as fast as possible for 30 sec.: thus △ △ △ △ △ △ ....

(*b*) A similar series was then drawn, with apex downwards, as fast as possible for 30 sec.: thus ▽ ▽ ▽ ▽ ▽ ▽ ....

(*c*) (*a*) and (*b*) were repeated.

(*d*) The subjects finally drew as rapidly as possible for 2 min. the following series △ ▽ △ ▽ △ ▽ ....

3. *Alphabet and number test.*

(*a*) The subjects had to write a b c d e f g a b c d e f g ..., and to continue this as fast as possible for 1 min.

(*b*) They were then told to write 1 2 3 4 5 6 7 1 2 3 4 5 6 7 ..., and repeat as fast as possible for 1 min.

(*c*) They were then instructed to mix (*a*) and (*b*), each letter being followed by a corresponding numeral, thus: a 1 b 2 c 3 d 4 e 5 f 6 g 7 ..., and repeat as fast as possible for 2 min.

4. *Mirror image test.*

(*a*) The subjects were instructed to write in block characters the letters B C D E F (and to repeat them) as fast as possible for 2 min.

(*b*) They were then told to write the same letters for 2 min. in the form they have when seen in a mirror, *i.e.* ᙠ Ɔ ᗡ Ǝ ꟻ, and to repeat them as fast as possible.

In each case the whole procedure was explained before any single test was started, so as to avoid a break between the first and second half or at any other place in the test. The necessity of keeping up the speed was emphasized, both at the beginning and throughout the procedure. The tests were applied five times after five practices, and the scores for perseveration were reckoned by subtracting the total number of letters and figures written (minus the errors made) in the second 2 min. (= y) from the total number of letters and figures written in the first 2 min. (= x). It will be noticed that no mention is made of any errors made in the first 2 min. for the simple reason that the test became possible only after all errors had been eliminated during the first 2 min. by previous practice.

IV. PERSEVERATION AS A GROUP FACTOR.

The results of the four tests applied five times after five practices to 194 children of both sexes between 8 and 15 years showed an average inter-correlation of 0·34, the entire table being as follows:

Table IV.

	S.	Δ	A.N.	M.I.
S.	—	0·34	0·29	0·37
Δ	0·34	—	0·37	0·33
A.N.	0·29	0·37	—	0·36
M.I.	0·37	0·33	0·36	—
Averages	0·33	0·34	0·34	0·35

When the tetrad differences for the above correlations are worked out (according to the formulae given in the appendix to Spearman's *The Abilities of Man*), the following figures result:

Mean deviation of tetrads from zero = 0·0263
Probable error (P.E.) = 0·02104
Theoretical P.E. = 0·0217

The probable error actually obtained is thus practically the same as that which we should expect theoretically to get; and this, according to Spearman's theory, confirms the presumption that a group factor of perseveration exists common, at least, to the four tests mentioned.

V. SEX AND AGE DIFFERENCES.

The differences in sex and age are shown in Table V, where

x stands for the average output of the first 2 min. work for the four tests repeated five times,

y stands for the average correct output of the second 2 min. work for the four tests repeated five times, and

P stands, as before, for $x-y$ (cf. p. 12), the average measure of perseveration.

Table V.

No. of subjects	Sex	P	$x+y$	Ages
121	Women	112	825	Adults
82	Men	151	871	,,
203	Men and women	131	848	,,
113	Boys	93	522	8–14 years
94	Girls	94	533	8–15 ,,
207	Boys and girls	94	528	8–15 ,,
13	,, ,,	108	670	14–15 ,,
29	,, ,,	104	637	13 ,,
26	,, ,,	107	625	12 ,,
33	,, ,,	99	608	11 ,,
35	,, ,,	86	470	10 ,,
39	,, ,,	79	367	9 ,,
32	,, ,,	63	320	8 ,,

From the above figures it appears that there is hardly any difference in P between boys and girls, but a fairly marked difference between men and women, and that there is a steady decrease in P from 12 to 8 years of age. (But this decrease may have been influenced by the slower speed at which the younger subjects worked.) For this reason men and women and children of different ages were always grouped and ranked separately for purposes of correlation with estimates of character, intelligence quotients, etc.

VI. Relation to types of character.

While the experiments on the tests were being carried out, the psychological investigators under the direction of Dr Hadfield studied the children at the Leytonstone Home with a view to classifying them under different types of character. The domestic staff soon entered into the spirit of the enquiry and maintained their interest throughout the work. They assisted more especially by writing short character studies of each of the children under their supervision and care. These sketches, it was thought, would be of great help to the psychological investigators in their study of the material they were experimenting on. In these sketches and during the discussions which followed certain designations were found to be constantly used—the words "difficult," "self-controlled," "reserved," "sociable," came up with almost monotonous regularity. It was therefore agreed that the staff should mark the children individually according to the degree in which each of these six characteristics was present or absent. The following was the scale of marks used: 0 stood for normal; + 1, + 2, + 3 marked the degree in which the characteristic was above normal; and − 1, − 2, − 3 marked the degree in which the characteristic was below normal.

This marking was carried out by two house-masters and by seven house-mothers, each supported by one or two assistants; the matron (who had had 2 years' psychological training), with the help of the psychological investigators, supervised and controlled this part of the work. The children had been in the institution for several years prior to this experiment and were well known to the staff. The process of observing and marking lasted over a period of 3 months.

The correlations between the estimates of character and the score for perseveration were as follows for the different groups of children (each group comprising both sexes):

Table VI.

	No. in group	Age	Difficult	Self-controlled	Reserved	Sociable	Perse-vering	Un-reliable
	29	13	−0·07	0·15	0·01	0·11	0·12	0·02
	25	12	0·13	0·17	0·14	0·10	0·17	0·21
	33	11	0·12	−0·12	0·23	−0·21	0·15	0·18
	35	10	0·23	0·00	0·27	0·13	0·17	−0·14
	30	9	0·15	0·03	0·11	0·07	0·04	0·09
	32	8	−0·24	0·11	0·08	0·16	0·03	0·13
Total	194	8–13	−0·02	0·06	0·14	0·06	0·11	0·08

It was obviously impossible to draw any conclusions from these results.

Before this stage in the work was reached, however, it had been generally commented on by the staff that the strongest and weakest perseverators were more 'difficult' and far less 'amenable to control' than the rest of the children taken together. To test this opinion, the domestic staff was supplied with a list of the characteristics mentioned above, and they were asked to mark the most outstanding characteristics for each of the 194 children under their care by writing a cross opposite the child's name under the characteristic which was specially prominent in him. The subjects were then arranged in the order of their perseveration (according to their performance of the tests) and they were divided into eight equal groups, the first group containing the strongest perseverators, the last group containing the weakest. The crosses relating to the members of each group were now transferred to it, and added together; and the average numbers of crosses for each characteristic were graphed in relation to the average perseveration for each of the groups. The outcome was a series of curves, which resembled each other fairly closely, with this difference that some were inverted. By decreasing the number of groups from eight to four it was found that certain curves were, practically speaking, inverted replicas of others.

The procedure was therefore now as follows. The average perseveration mark of the 194 children was taken, then the averages of those marks which fell (*a*) above, and (*b*) below that mark were calculated respectively. By these three dividing lines the subjects were segregated into four groups according to their marks for perseveration. The strongest perseverators fell in the group with the highest marks and were called Extreme Perseverators (E.P.); they were followed by the Moderate Perseverators (M.P.), the Moderate Non-Perseverators (M.N.), and finally by the Extreme Non-Perseverators (E.N.), which was the group with the lowest marks for perseveration. The crosses for the different characteristics in each of the four groups were totalled and the results tabulated. The following table shows the results of this classification.

Table VII.

	No. in each group	Degree of perseveration	Difficult	Self-controlled	Reserved	Sociable	Persevering	Unreliable
Boys (113)	27	E.P.	23	5	6	22	6	22
	31	M.P.	4	24	9	19	19	9
	29	M.N.	8	20	12	16	20	8
	26	E.N.	20	5	7	18	4	21
Girls (81)	19	E.P.	13	7	10	10	4	16
	21	M.P.	6	13	5	14	13	6
	17	M.N.	6	13	7	13	13	7
	24	E.N.	19	3	6	14	5	16
Boys and girls (194)	46	E.P.	36	12	16	32	10	38
	52	M.P.	10	37	14	33	32	15
	46	M.N.	14	33	19	29	33	15
	50	E.N.	39	8	13	32	9	37

These figures speak for themselves. Both for the boys (113) and girls (81) separately and for the two groups combined (194), the following points are perfectly clear. On an average 75 per cent. of the most difficult and unreliable children fall under the extreme forms of perseveration, while about 25 per cent. group under the moderate forms of perseveration. Exactly the opposite holds for the self-controlled and persevering children. The following are the actual figures.

Table VIII.

No. of subjects	Sex	Degree of perseveration	Difficult (1)	Self-controlled (2)	Reserved (3)	Sociable (4)	Persevering (5)	Unreliable (6)
53	Boys	E.P. & E.N.	43	10	13	40	10	43
60	,,	M.P. & M.N.	12	44	21	35	39	17
43	Girls	E.P. & E.N.	32	10	16	24	9	32
38	,,	M.P. & M.N.	12	26	12	27	26	13
96	Boys & girls	E.P. & E.N.	75	20	29	64	19	75
98	,, ,,	M.P. & M.N.	24	70	33	62	65	30

Applying the formula $\epsilon^2_{12} = \frac{p_1 q_1}{n_1} + \frac{p_2 q_2}{n_2}$, given by Yule (3) to prove or disprove the significance of the above figures, the following results are obtained:

p_1 is the proportion of the cases of the one kind, p_2 is the proportion of the cases of the other kind, n_1 is the number of the cases used to get p_1, and n_2 is the number of cases used to get p_2:

$$q_1 = 1 - p_1, \quad q_2 = 1 - p_2.$$

Results for the six characteristics in the order given in the previous table.

(1) P.E. = 0·04; $p_1 - p_2 = 0{\cdot}53$, which is 13 times as large as P.E.
(2) P.E. = 0·04; $p_1 - p_2 = 0{\cdot}52$, which is 13 times as large as P.E.
(3) P.E. = 0·05; $p_1 - p_2 = 0{\cdot}04$, which is less than P.E.
(4) P.E. = 0·05; $p_1 - p_2 = 0{\cdot}02$, which is less than P.E.
(5) P.E. = 0·04; $p_1 - p_2 = 0{\cdot}47$, which is nearly 12 times as large as P.E.
(6) P.E. = 0·04; $p_1 - p_2 = 0{\cdot}46$, which is nearly 12 times as large as P.E.

The figures for (1), (2), (5), (6), are definitely significant and those for (3) and (4) are not significant at all. Whence we conclude that self-control and perseverance go with moderate perseveration, while the 'difficult' character and unreliability go with extreme perseveration and extreme non-perseveration. No relation of this nature occurs in the case of reserved and sociable children.

The chief objection to the interpretation of these figures may be the possibility that unwillingness and lack of effort have been measured by the tests rather than perseveration, although it would not affect the significant differences between the figures themselves. The experimenter must admit that when first he studied these figures he himself could not get rid of that suspicion. In the first place, as has been explained above (pp. 10, 11), unwillingness and lack of effort might and did account for results which could be interpreted as showing extreme forms of perseveration; and secondly, it would naturally follow that the unwilling child would be ranked as difficult and unreliable, and that the willing children might often be considered reliable and self-controlled. On the other hand, it must be said that such special efforts were made to do away with unwillingness and to substitute some positive motive for doing the tests and ultimately with so much response on the part of the subjects, that one could hardly feel justified in following the above line of argument, although psychologically it seemed the most obvious.

The next procedure was to supply the staff with a list of the names of the children in the four different groups and get from the staff the differences and similarities in character between the groups. There was general agreement that the extreme groups taken together were composed of the 'difficult' children as compared with the two moderate groups, but that there was a marked difference between the extreme groups taken separately. The E.P. (extreme perseverator) group was considered to comprise the real rebels, who were irritable, bad-tempered and moody. These, as a group, were the leaders in mischief of the E.N. (extreme non-perseverator) group, who were 'petty,' nagging and whining and were always happiest in a crowd. The differences between the M.P. and M.N. (moderate perseverator and non-perseverator) groups were less marked, although the M.N. group was considered more self-confident, jovial and popular than the M.P. group. It is a remarkable fact that of the twenty-four prefects in the six houses of the Leytonstone Home, seventeen belonged to the M.N. group, five to the M.P. group, and two to the E.P. group. Of these the five in the M.P. group were considered more reliable and better leaders from the staff's point of view than the other groups. The two prefects in the E.P. group were in constant conflict with the staff[1].

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[1] The opinion of the staff as regards the differences between these groups was almost wholly corroborated by the writer's later experimental work on "Perseveration and Introversion." The one exception related to irritability, which proved greatest among the extreme non-perseverators. But for this, perhaps, differences of definition were responsible.

In the face of these facts it seemed hardly possible that any unwillingness or lack of effort could account for the far-reaching differences in character between the extreme groups, and especially between the moderate groups which certainly could not be affected at all by the factors mentioned above.

VII. CONFIRMATION BY LATER EXPERIMENTS.

The last vestige of doubt on this matter was removed by the results of the same experiment carried out on adults at the Maudsley Hospital, where the great majority of patients were extremely willing and even anxious to do the tests, and where out of a total of 116 patients very few indeed were extremely depressed and even they with a little encouragement did their best. Consequently it can be said with fair certainty that the degree to which unwillingness entered into the tests at the Maudsley Hospital was relatively slight, and that it could never have been a determining factor to anywhere near the same degree as was possible in the Leytonstone experiment. Inasmuch as the proportion of the different groups to each other is about the same in the Maudsley as in the Leytonstone results, the conclusion is justifiable that perseveration and not unwillingness was the determining factor and responsible for the particular form of grouping.

The 116 patients examined at the Maudsley Hospital were spread over four wards, two for the men and two for the women, and they were observed and marked for four characteristics by the four respective sisters of the wards in collaboration with the rest of the nursing staff. Most of these patients were well known to the staff and the work lasted over a period of 9 months. The following are the figures.

Table IX.

	No. in each group	Degree of perseveration	Difficult	Self-controlled	Persevering	Unreliable
Men (48)	13	E.P.	10	1	3	9
	12	M.P.	4	9	7	4
	10	M.N.	5	8	7	2
	13	E.N.	10	2	1	10
Women (68)	16	E.P.	9	2	1	8
	14	M.P.	1	6	7	2
	19	M.N.	2	3	5	3
	19	E.N.	6	1	1	9
Men and women (116)	29	E.P.	19	3	4	17
	26	M.P.	5	15	14	6
	29	M.N.	7	11	12	5
	32	E.N.	16	3	2	19

Grouping the figures in the same way as for the Leytonstone experiment, the following table results.

Table X.

No. of subjects	Sex	Degree of perseveration	Difficult	Self-controlled	Persevering	Unreliable
26	Men	Extremes	20	3	4	19
22	,,	Moderates	9	17	14	6
35	Women	Extremes	15	3	2	17
33	,,	Moderates	3	9	12	5
61	Men and women	Extremes	35	6	6	36
55	,, ,,	Moderates	12	26	26	11

Here again, as in the former experiment, there is a definite system of grouping. On an average 75 per cent. of the most difficult and unreliable patients fall under the extreme forms of perseveration, whereas the opposite holds for self-control and perseverance in about the same proportion. For none of the above characteristics is $p_1 - p_2$ less than eight times the probable error; so that the figures must be considered definitely significant.

VIII. SUMMARY.

Four tests of perseveration have been used in this investigation and applied to 194 children and 116 adults.

According to the Spearman formula of tetrad differences, a group factor, presumably of perseveration, exists, common at least to these four tests.

There appears to be a steady increase in the mark for perseveration among children of increasing age, but little difference between boys and girls. Adults show a much higher mark for perseveration than do children, and men a somewhat higher one than women. But differences in speed of work need here to be taken into consideration.

About 75 per cent. of the most 'difficult' and 'unreliable' subjects proved to be extreme perseverators or extreme non-perseverators. About 75 per cent. of the most 'self-controlled' and 'persevering' subjects showed only a moderate degree of perseveration.

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SOME PSYCHOLOGICAL EXPERIMENTS ON MENTAL DEFECTIVES IN RELATION TO THE PERCEPTUAL CONFIGURATIONS WHICH MAY UNDERLIE SPEECH. I.

By R. G. GORDON and R. M. NORMAN.

(*From the Stoke Park Colony for Mental Defectives*[1], *Bristol.*)

I. *Introduction* (pp. 20–21).
II. *Head's aphasic tests* (pp. 21–33).
  (1) *Preliminary observations* (pp. 21–22).
  (2) *First test group* (pp. 22–24).
  (3) *Second test group* (pp. 24–27).
  (4) *Third test group* (pp. 27–29).
  (5) *Fourth test group* (pp. 29–30).
  (6) *Fifth test group* (pp. 30–31).
  (7) *Sixth test group* (pp. 31–33).
III. *Delayed reaction experiments* (pp. 33–41).
  (1) *Puzzle-box tests* (pp. 33–39).
  (2) *Speech tests* (pp. 39–41).

I. INTRODUCTION.

THE material used for these investigations consisted of two groups of defectives. The first consisted of a hundred high-grade females, all of whom had an intelligence quotient above 40, and a hundred low-grade males, all of whom had an intelligence quotient under 30. The first three sections of this paper and its continuation deal with the abilities of mentally defective persons in the domain of speech and spatial manipulation, while the fourth is concerned with imagery. Before describing these experiments, it may be as well to draw attention to certain features of mental defectives which make their study peculiarly interesting from the psychological point of view. In the first place, in the majority of cases the defect is due to an arrest of development which is germinal in origin and not due to intercurrent disease, so that the oligophrenia, or 'small wittedness' as Continental writers call the condition, is due to an insufficiently developed brain which grows abnormally slowly, but in a fashion not incomparable with the normal, and is not actively degenerating as a result of any disease process.

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[1] The research scheme initiated by the late Dr R. W. Branthwaite, first Director of Medical Services at Stoke Park Colony, has been continued and greatly amplified by Prof. R. J. Berry, the present Director of Medical Services, with the active and liberal support of the late Warden, the Rev. H. M. Burden, and the present Warden, Mrs R. G. Burden, with the approval of the Board of Control. The research laboratory has been completed and refitted, and there has been appointed a medical consultant and research staff. A complete mental, medical, biochemical, neurological and psychological examination of the patients is now being systematically made, and this paper is the first contribution to the researches now in operation.

Secondly, this brain is so poorly developed in the lower grade cases as to bring the resultant mind into comparison with that of higher animals, and so serve as a link in mental development between the anthropoids and *Homo sapiens* in his full development. On the other hand, while mental defect is as a rule not a defect of intellect or mental development alone, but represents a general defect in development, the bodily defect may be insignificant compared with the mental defect, so that the individual may exhibit the mind of a child but the bodily and endocrine development of the adult, and therefore possibly possess temperamental characteristics disproportionate to intellectual and integrative qualities.

II. HEAD'S APHASIC TESTS.

(1) *Preliminary observations.*

The first of our experiments was to apply certain of the tests devised by Sir Henry Head for his aphasic cases to these two groups (1). These tests were carried out by Miss J. Worger, assisted for the second group by Miss M. Havers. Head claims that the aphasias are lesions of symbolic formulation and expression. These functions are high-grade 'mental' functions far above those of movement on the one hand or of vision and hearing on the other. "Speaking, reading and writing are highly developed actions which have been acquired during the life of the individual. They are learnt with effort and constantly improved with practice. When they are disturbed by an organic lesion, the orderly march of a series of events is interrupted; the act of speech fails in one way or another, assuming different forms....The necessary processes can no longer be carried out perfectly, but the particular form assumed by the disorder is the consequence of preventing in part the expression of certain highly developed functions and does not reveal the primitive elements out of which they are developed." In other words speech involves a higher

P16452

integration involving both afferent and efferent processes, but something more than an algebraic summation of these. "By symbolic formulation and expression I understand a distinct mode of behaviour in which some verbal or other symbol plays a part between the initiation and execution of the act." "It is not the general intellectual capacity which is primarily affected by the loss of this function, but the mechanism by which certain aspects of mental activity are brought into play."

If this function is deficient, the higher the propositional value of the test applied, *i.e.* the more complex the configuration, the more difficult will be its execution; thus "a patient may execute a command to hold up his hand, although he is unable to carry out an order to touch with it his eye or his ear." "Any modification of the task which lessens the necessity for symbolic representation, will render its performance easier. Thus, a patient who finds extreme difficulty in imitating gestures accurately, sitting face to face, makes no mistake when they are reflected in a mirror, for this is in most cases an act of direct imitation (2)."

Here, then, we have a high-grade function not entirely dependent on general intelligence, but demanding a considerable degree of integration of simple material into more complex configurations which are themselves new. The power to form such configurations was easily upset by comparatively small cortical lesions as Head demonstrated in his war cases, though these lesions represented interference with association links and not destruction of special centres. It seemed interesting to discover the working of this function of integrating and manipulating such configurations in those with imperfectly developed brains. For this purpose six groups of tests as devised by Head were employed. Of these groups of tests, three, I, IV and V, are apparently tests of the manipulation of simple word configurations, while the other three, II, III and VI, were tests of less familiar spatial manipulation as described below. The tests were purposely applied in the order I to VI in sequence so as not to segregate one type of test from the other.

(2) *First test group.*

The first of these groups comprised a testing of the defectives' reactions to certain familiar objects, viz. *knife*, *key*, *penny*, *matches*, *scissors*, *pencil*, and except for the first matching test, was a pure word manipulation test. Thus, the first demand was a simple matching test, the subject being asked to point to an object on the table corresponding to the one shown to him. This matching is easy, and Head has shown that it can always be performed by aphasics. The subject uses one sense, namely, vision, both in the receptive and responsive part of the test, and the intervening image configuration necessary is probably quite vague and inexact. Next he is asked to point to the object named verbally by the observer, so he has to receive by hearing, 'translate' the word into its meaning and respond by vision and movement. Thirdly, he has to point to an object named in print. Here he has to receive the visual impression of written speech, translate this into its meaning and respond by vision and movement.

Next the duplicate is placed in his hand out of sight and the subject has to receive through the less usual kinaesthetic sense, responding by vision and movement. Lastly, he must write the name of the object indicated, receiving by vision and responding by the complicated kinaesthetic-motor integration of writing.

In submitting this test to defectives, it is obvious that its full success will depend on the ability to read and write, which is not possessed by all our subjects even in the first high-grade group. Leaving out of account spelling mistakes, provided the word written obviously corresponds to the object indicated, we find that in the first series of 100 females with an I.Q. over 40, 78 per cent. completely succeeded in the test, 14 per cent. succeeded except in so far as the ability to write was concerned, and 8 per cent. except in so far as the ability to read and write. So far as these high-grade defectives were concerned, therefore, there was no difficulty in actual performance of this group of tests, failure being due to lack of other capacities, due at any rate in some cases to educational rather than to inherent defect. It would seem, therefore, that these people can form and work with configurations which represent familiar objects, or the names of these.

In the second group of cases, it was found that 33 were unable to perform any of Head's tests at all. These were the idiots and lower grade imbeciles.

Of the remaining 67, two only were able to do this test entirely, due allowance being made for spelling mistakes. Two others (encephalitic cases) did it perfectly, but could not write owing to the tremor. One other did the first five divisions of the test perfectly, but failed in the writing test. Another succeeded to the same degree except that he failed to recognize the meaning of the printed word knife and point to it.

Sixty-one cases could not read or write, and of these 35 did the rest of the test correctly. Nine did the four divisions with only minor inaccuracies. Two could not speak, so failed in the naming tests, but quite a reasonably high proportion were able to cope with this group of tests.

Twelve found difficulty in the kinaesthetic tests in which the subject is required to point to an object the duplicate of which is placed in his left hand out of sight, and which may mean that a kinaesthetic image is not easily formed in these low-grade cases. Of these 12, seven succeeded in the pointing and naming tests, but failed in the kinaesthetic test altogether. Two attempted this test but made serious mistakes. Three others showed mistakes in the pointing and naming tests as well, although the latter were reasonably performed. Lastly, one succeeded in the two pointing tests, but owing to inability to speak could do no more. One could point to the object shown to him, but not if named by the observer, and could name the object himself, while one could only name the objects.

It is obvious from this that this group of tests is reasonably easy for most of even this low group provided that the tests can be performed at all, and the subjects can form and use these word configurations.

(3) *Second test group.*

The second group of tests is of especial interest in view of its recent application to normal people by Charles Fox (4). These are the finger-eye, finger-ear tests. In this group of tests the subject is asked to imitate the observer in moving the left hand to the right eye, right hand to right ear and so on in different combinations. In order to give the subjects encouragement in what might prove a difficult test, we started the test with a direct imitation—the subject imitating the movements of the observer seen in the glass; this is comparable to direct matching. Next the subject was asked to imitate the observer when facing him, thus having to respond with the opposite hand; then a picture of the movement was presented and the subject had to copy it, first facing it, then as seen in the glass (*i.e.* direct matching). Head (3) thinks that the copying of a movement when facing the observer or the picture requires the intervention of language. Fox has pointed out from the introspective records of his subjects that there are three possible ways of solving the problem. The subject may give himself a verbal order by the use of 'internal speech,' such as "I must do the opposite," or "I have to use the opposite hand," or "The left hand on the picture corresponds to my right"; and this was the method used by some of his graduate subjects. But the problem may be tackled by spatial manipulation only, without the use of internal speech, and this may take various forms. The person may imagine that the diagram is turned round, and consider himself as looking through the card from the back as though it were transparent. Or he may imagine the card placed behind him so as to face the same way as he himself is looking. It would usually be said that this involves the use of visual imagery, but if any imagery is employed it is so vague as hardly to be recognizable. The ability to think of objects as turned round is acquired so early in life that, if images were originally used, they have long since disappeared from consciousness. For images tend to disappear as soon as they have accomplished their purpose of making thinking facile; and when thought flows freely images play no part in the process. We may describe this procedure of spatially manipulating the card in imagination as 'orientation of the diagram.' Finally, it is possible to solve the problem by means of orientation of the body, and this may be accomplished in a variety of ways which it is difficult to separate. The more usual method involves the use of kinaesthetic imagery or incipient bodily movements. The person imagines his own body taking up the position represented on the picture. It is very doubtful whether pure kinaesthetic imagery has any independent existence, or whether in all such cases incipient bodily movements are alone involved. The spatial schema of our body, which we all carry about with us in consciousness, is more or less easily manipulated. Some subjects aver that they 'feel' their bodies turned round in space so as to be in the position of the picture. Others 'feel' nothing, but simply 'know' that their bodies have made the necessary turning movement. Careful observation of the subjects discloses the fact that incipient movements of turning the body are made; and in some cases the movement is very pronounced, so as to constitute a quarter or half a turn of the body in the chair. This is more especially the case with children, and I have observed some schoolboys who actually got up and turned completely round in their effort to solve the problem. The orientation sometimes takes a visual form, and the subject 'sees' himself turned so as to occupy the position of the man in the sketch. For purposes of discussion it is convenient to refer to all the cases of this last group, namely kinaesthetic, incipient or actual movements, or visual realization of movement, as 'orientation of the body.' The above analysis is based on the actual procedure adopted by the persons who were examined. It is to be noticed that to give the defective cases every chance, we started with the simple imitative test which they almost all did correctly, and as Fox points out this ought to reduce the errors very materially, yet in the whole series of cases only four were able to make a complete success of this test.

This is in accordance with Fox's findings that this test cannot be performed under 9 years even by the brightest children. We would agree with Fox that this test and probably the clock test, which we used as the third of our group of tests, are concerned with the ability of the individual to discriminate and manipulate spatial ideas, and that with Mourgue (5) we may conclude that speech defects are defects in a more general function of ability to demarcate or differentiate perceptual configurations, which shows itself in a lack of appreciation of spatial relationships. Mourgue thinks that the varieties of aphasia are reducible to special disabilities in spatial discrimination, that is in the demarcation of perceptual configurations in space, and that language is only disturbed because it too involves such demarcation (p. 123). Such a function of discrimination and of space relationship clearly depends on high level cortical function, and therefore in the case of defectives, who are in all probability wanting in the higher layers of the cortex, it might be expected that these functions would be lacking. This group of tests comprises the following:

Hand to eye and ear group.

(1) Imitation of movements of the observer seen in glass. 14 tests. (Pure imitation —easy.)
(2) Imitation of movements made by observer. 14 tests. (Transposition—difficult.)
(3) Carrying out of pictorial commands. 14 tests. (Transposition—difficult.)
(4) Carrying out of pictorial commands seen in glass. 14 tests. (Imitation—easy.)
(5) Carrying out oral commands. (Language test—auditory.)
(6) Carrying out printed commands. (Language test—visual.)
(7) Reading aloud and executing printed commands. (Language test—auditory and visual.)
(8) Writing down movements made by observer. (Transposition and motor language.)

In this group of tests the following results were obtained for the 100 high-grade girls. There were only four complete successes. The really interesting tests in this group are the first four and the last. As has been seen, tests (1) and (4) should be easy, and (2), (3) and (8) difficult. In spite of this we find 29 having errors in (1) and (4), a much higher percentage than the imitation tests involving pure word manipulation, presumably because these spatial configurations were less familiar than the word configurations. When we come to the difficult tests, we find that there are no less than 94 who made errors in these tests, (2), (3) and (8). Of these, 36 failed to score any successes in any of them. For some reason (8) was done much more successfully than (2) and (3), possibly because the subject was more used to the tests by the time (8) was reached. Thus, there were 14 responses required for each test. Of the 94 subjects under discussion:

For test (2).
84 had 14 errors.
1 had 13 errors.
1 had 10 errors.
1 had 7 errors.
1 had 4 errors.
2 had 3 errors.
4 had 0 errors.

For test (3).
83 had 14 errors.
2 had 12 errors.
1 had 11 errors.
1 had 10 errors.
2 had 6 errors.
2 had 5 errors.
1 had 3 errors.
2 had 0 errors.

It is obvious then that these two tests proved very difficult indeed. For test (8) we find:

Only 38 with 14 errors.
1 with 13 errors.
1 with 12 errors.
4 with 11 errors.
2 with 10 errors.
3 with 9 errors.
4 with 7 errors.
2 with 6 errors.
1 with 4 errors.
4 with 3 errors.
6 with 2 errors.
5 with 1 error.
And no less than 23 without errors.

These results show a close analogy with Fox's for normal school children.

In the second class, 33 could not do Head's tests at all. None were completely successful in the second group of tests, and for the difficult tests, none reached test (8), as the whole business was too complicated. For tests (2) and (3), 50 entirely failed in both these tests.

Of the 67 who attempted the tests at all:

For test (2). 55 had 14 errors.
7 had 13 errors.
2 had 12 errors.
1 had 11 errors.
1 had 10 errors.
1 had 8 errors.

For test (3). 58 had 14 errors.
3 had 13 errors.
1 had 12 errors.
2 had 11 errors.
1 had 10 errors.
1 had 6 errors.
1 had 2 errors.

It is obvious that these tests are very difficult indeed for these persons. It should be noted however, that whereas the higher group were fairly successful in tests (1) and (4), only 29 per cent. having inaccuracies, the results in the lower group were very inaccurate indeed, though definitely better than for tests (2) and (3). Only five were accurate in both (1) and (4), while one was accurate in (1) with one mistake in (4) and one was accurate in (4) with two mistakes in (1).

This test is then of considerable difficulty for low-grade cases, the transposition being almost impossible.

(4) *Third test group.*

The third group of tests consists in clock setting—of these there are ten "times" used.

(1) This consists in direct imitation of the hands on another clock. The only difficulty here is a confusion of the big and little hands.

(2) The clock is set to oral command.
(Auditory impression translated to motor movement with the intervention of a symbolization of the time.) This test demands the ability to tell the time.

(3) The clock is set to printed command.
(The same as the last but the reception is by vision and not hearing.)

(4) Telling the time from a clock set by the observer.
(A visual reception and a transference to speech involving the knowledge of telling the time.)
(5) Writing the time shown on a clock set by the observer.
(The same as (4) but involving the ability to write.)

Of the first series of 100 high-grade girls, 23 succeeded in passing the group of tests without error. Twenty-five failed in groups (2), (3), (4) and (5) because they could not tell the time. Of these, ten made no errors in the direct imitation group of tests. From the 15 who failed even to imitate correctly, there were 150 possibly correct solutions, since there are ten responses asked for each of these tests. There were 53 errors out of the 150 responses. Of these, 28 were simply transpositions of the big hand for the little hand, while 25 were definite errors. The 52 who could tell the time gave 2540 responses, since there were 60 refused responses. Of these, 424 were erroneous and of these 75 were transpositions of big hand for little hand and 349 true errors. It will be seen from this, therefore, that there was quite an appreciable difficulty in spatial manipulation amongst this group of defectives as demonstrated by the clock test.

For the lower-grade group this proved a very difficult test indeed. In addition to the 33 who could not do Head's tests at all, 34 could not attempt this test. A further 28 could only attempt the direct imitation test while only five got farther than this. Of the 28 who only did the imitation one had no errors. One had one error only which was a reversal and nine successes.

1 had 1 error, 1 reversal and 8 successes.
1 had 3 errors, 2 reversals and 5 successes.
1 had 4 errors, 2 reversals and 4 successes.
2 had 5 errors, 1 reversal and 4 successes.
1 had 6 errors, 0 reversals and 4 successes.
1 had 2 errors, 5 reversals and 3 successes.
1 had 6 errors, 1 reversal and 3 successes.
2 had 7 errors, 0 reversals and 3 successes.
1 had 3 errors, 5 reversals and 2 successes.
1 had 4 errors, 4 reversals and 2 successes.
7 had 8 errors, 0 reversals and 2 successes.
1 had 2 errors, 7 reversals and 1 success.
1 had 8 errors, 1 reversal and 1 success.
3 had 9 errors, 0 reversals and 1 success.
1 had 8 errors, 2 reversals and 0 success.
1 had 10 errors, 0 reversals and 0 success.

That is, there were only 83 successes out of a possibility of 280, while there were 164 complete errors out of 280. One case attempted the imitation test scoring three successes, three reversals and four errors, and also the setting the clock to oral command, in which he scored two successes, one reversal and seven errors. Another was accurate in the imitation test and only made one error in telling the time from the clock, but could not do the other tests. Another was accurate in both the above tests. One case did the imitation test with nine successes and one reversal and was correct in setting the clock and telling the time. The last case did all the tests except writing down the time, but was inaccurate in the imitation test, scoring three successes, five reversals and two errors. In normal people, while reversals are not uncommon, complete errors are rare. The high proportion of complete errors seems to indicate that these cases have a difficulty in retaining spatial forms at all and this is borne out in the next group of tests dealing with retention of spatial configurations.

(5) *Fourth test group.*

The fourth test is a word configuration test and consists in the exhibition of a series of pictures and captions representing three figures differently grouped. Thus:

(1) The dog and the cat.
(2) The man and the dog.
(3) The cat and the man.
(4) The cat and the dog.
(5) The dog and the man.
(6) The man and the cat.

The tests required were:

(1) To read aloud from the caption.
(2) To read from the pictures.
(3) To write from the pictures.
(4) To write from dictation.
(5) To repeat the caption dictated.
(6) To read what he has written.
(7) To copy the caption.

Here the subject is required to receive two configurations in different ways and to convey them in different ways, except in the last case when he is only required to copy, and is not required to attach any 'meaning' to the configurations. It is possible that meaning need not be attached in tests (1), (2), (5) and (6), but it certainly must be attached in tests (2) and (3), and from the point of view of mental achievement (2) and (3) are presumably the most difficult. In order to score full marks at this test, it is not only necessary to interpret the figures for what they are, *i.e.* man, dog or cat, but to describe them in their proper order. Thus picture (1) must be described as dog and cat and not cat and dog.

In the first series 32 per cent. of the high-grade girls were absolutely correct. Of the remaining 68 per cent. all made mistakes in columns (2) and (3), and 60 made mistakes only in these columns. Of the remaining eight, three failed as a result of complete inability to read and write, others of course being able to read or write such simple words as man, cat, dog, when they could not succeed with colours or objects, such as scissors, knife, etc.

Of the rest, one inverted the order in one test when writing from dictation. In one, *dog* was read as *God* and in two others written as *God*. In one, *man* was read as *mouse*.

In the whole series, there were 12 who showed errors in interpretation.

Thus:

(1) Writing from picture, man and dog.	Response was man and cat.
(2) Reading from picture, dog and cat.	Response was tiger and sheep.
(3) Reading from picture, dog and cat.	Response was cat and tiger.
(4) Reading and Writing from picture, dog and cat.	Response was cat and lion.
(5) Reading from picture, dog and cat.	Response was two cats.
(6) Reading from picture, dog and cat.	Response was lion and cat.
(7) Reading from picture, dog and cat.	Response was cat and tiger.
(8) Reading and Writing from picture, dog and cat.	Response was wolf and squirrel.
(9) Reading from picture, dog and cat.	Response was wolf and cat.
(10) Reading from picture, dog and cat.	Response was cat and squirrel.
(11) Reading and Writing from picture, cat and dog.	Response was cat and man.
(12) Reading from picture, cat and man.	Response was dog and man.

It is to be noticed that in ten of these the error was in the first test, the responses afterwards being accurate, so that this may be of less significance than at first sight appears. We may conclude, therefore, that these high-grade cases have little difficulty with this test of forming and manipulating these simple and familiar configurations.

The crux of this test is the reading from pictures, and although this was inaccurately performed amongst the low-grade cases, only one being quite accurate, only four besides the 33 who could do none of the Head's tests discussed in this paper were unable to make quite a reasonable attempt at this test. There were a good many wrong interpretations, but these were of the same type as enumerated above, and it does not seem worth while to set these out in detail. This test therefore was not found remarkably difficult by either class.

(6) *Fifth test group.*

An almost identical group of tests to the first is No. V in the series. This consists in giving various responses to colours shown twice in varying order. The colours are black, red, blue, green, orange, white, violet, yellow and the responses are as follows:

(1) Pointing to similar colour to that shown.
(2) Pointing to colour named by observer.
(3) Pointing to colour named in print.
(4) Naming colour shown.
(5) Writing name of colour indicated.
(6) Printed name read aloud.

Allowing for deficiencies in reading and writing, 97 per cent. of the first class succeeded in these tests, and some apparently found the names of colours more easy to read and write than the names of objects. The three who made significant mistakes were all alike in having difficulty in distinguishing violet from yellow or orange. In none of the three was this constant all through the tests, but evidently the distinction was not clear. There were no cases of the much commoner red-green colour blindness.

In the second group of cases this test like the other word configuration tests was relatively easy. Thus besides the 33 non-starters, only two made no attempt at this test while, allowing for inability to read and write, 25 performed the rest of the tests correctly. The remainder (40) were inaccurate, 15 showed complete confusion of colours in naming or pointing to colours named, 21 confused orange and violet, four confused blue-green, three blue-red, three yellow-green, two blue-yellow.

Of the 65 who attempted the tests, 55 were accurate in the plain matching test, eight who failed were of the class who were completely confused as to colours, one could not distinguish green-blue and one orange-violet. These colour confusions are interesting, but so far they have not been studied in sufficient detail to allow any conclusions to be drawn.

(7) *Sixth test group.*

The sixth group of tests is a test of spatial discrimination and manipulation. It consists in asking the subject to respond to a command involving two separate movements carried out simultaneously. Sixteen tests are given involving four separate commands repeated in different order. The four commands are:

(1) Give me your hand and put out your tongue.
(2) Shut your eyes and put out your tongue.
(3) Put out your tongue and give me your hand.
(4) Give me your hand and shut your eyes.

It is to be noticed that (1) and (3) are the same combination in different order and that only three simple movements are involved altogether.

In this test the subject must understand the order, he must discriminate the two movements required from the third or others, and he must integrate the two discriminated movements so that they may be performed simultaneously. This seems to be a good test for efficiency in the higher cortical functions in relation to movement.

Of the first class of 100 high-grade females, 39 succeeded in performing the 16 commands without error. None failed in all the tests and the prevailing error amongst the remaining 61 was failure to discriminate the two required movements, so that in some of the tests all three movements were carried out. However, these mistakes were relatively infrequent, and in 11 cases only one mistake was made out of the 16 tests. In only three cases was a command not obeyed, and in these cases only one each in the 16 tests. Thus:

(1) Shut your eyes and put out your tongue.
Response: Eyes shut and hand given.
(2) Give me your hand and put out your tongue.
Response: Hand given and eyes shut.
(3) Put out your tongue and give me your hand.
Response: Eyes shut and hand given.

It may be remarked, therefore, that this test was carried out fairly successfully by this class, but the chief difficulty lay in inhibiting a movement so as only to perform two when three were possible.

The test proved very difficult for the second group of cases. In addition to the 33 who could not do any of the Head's tests discussed here, two made no attempt. Of the remaining 65:

1 had no errors.
2 had 1 error.
5 had 2 errors.
3 had 3 errors.
8 had 4 errors.
5 had 5 errors.
7 had 6 errors.
5 had 7 errors.
4 had 8 errors.
7 had 9 errors.
2 had 10 errors.
9 had 11 errors.
3 had 12 errors.
1 had 13 errors.
1 had 14 errors.
1 had 15 errors.
1 had 16 errors.

That is, only 577 successes out of a possible 1040. 55 showed a failure of inhibition. 39 showed wrong order or mistake in carrying out the test, *e.g.* giving hand instead of shutting eyes. 20 only performed one movement instead of two.

This whole series of experiments shows that while language as a whole is difficult for defectives, simple word manipulation is easier than manipulation of configurations involving less familiar patterns of spatial orientation and control. The function of reference in space is a difficult one for those with a deficient cortex, and this is further borne out in subsequent experiments. Verbal images are actually used for this purpose in the delayed reaction experiments, thus confirming that word manipulation is the easier of the two problems. We next proceeded to test powers of spatial orientation and manipulation by more direct experiments on the forming and manipulation of spatial configurations.

III. DELAYED REACTION EXPERIMENTS.

(1) *Puzzle-box tests.*

In these simpler experiments on spatial manipulation and retention of spatial configurations we had an opportunity of comparing the behaviour of defectives with that of lower animals, as we employed a modified form of multiple choice apparatus. As used by Yerkes (6) the principle of the experiment is essentially that the animal is offered a choice among a number of open doors displayed at one end of the cage. Food is found in one of these, and this correct choice is made more or less difficult in a series of experiments, by varying the relation of the 'correct' door to the other doors, or increasing the total number of open doors, thus testing the animal's power of appreciating and using spatial relationships. Crows, for example, mastered the 'first on the right,' but failed in the 'second on the left,' even after 500 trials. Two monkeys tested by Yerkes (7) showed considerably greater capacity than other animals tested, and the existence of memory ideas of position can hardly be denied to Köhler's chimpanzees, who having seen fruit buried at night in the sand outside their cage, ran at once to the place when liberated in the morning, although the sand had been smoothed and no landmark marked the spot (8). With animals, therefore, the recognition of a place may be easy so long as they have not got to discriminate or manipulate a changed configuration, or one of too great complexity.

In the first try-out of this experiment, three boxes similar to those used as puzzle boxes were used. The glass top and front were covered in with paper, the middle red, the others white.

Twenty-three subjects from the first class (high-grade girls) were tested first. Each saw a chocolate dropped into one of the white boxes. After several days, averaging seven, the subject was reconfronted with the boxes and asked which one contained the chocolate. Nothing was said on the first occasion about coming back later.

The results were as follows:

15 correct.
8 incorrect. (Of these 6 chose the red box.)

In another series, four white boxes placed either side by side or one on top of the other were used.

The average interval before return in this group was 14 days. The chocolate was placed in either the second or the third box. Out of 47 tested in this way:

37 subjects gave correct responses.
10 subjects gave incorrect responses.

It soon became obvious that these experiments were not strictly comparable to those with lower animals, as language was of great assistance to the human subjects. Not only did they all come to learn from their friends what the procedure was about and that they had to try to remember which box the chocolate was in, but many evidently used a verbal phrase to assist them. Thus it was not unusual for a subject to reply "it's in the second one" or "the one from the bottom" without pointing until they were requested to do so. In some cases it was observed that counting began from right to left, so that what appeared an incorrect answer when spoken proved to be correct on pointing, showing that transposition of spatial relationship into words was difficult.

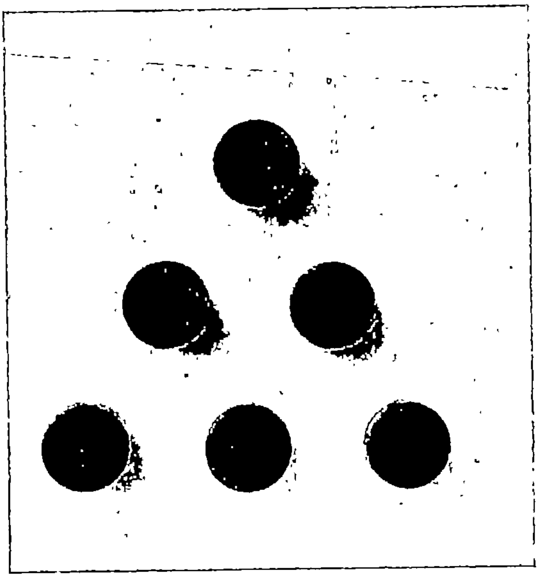

Fig. 1.

In order to make this use of a verbal mnemonic more difficult, a new apparatus was devised consisting of a number of black circular boxes which could be arranged in any desired position on a perpendicular white background. Six boxes arranged in the form of an equilateral triangle were used (see Fig. 1). The subject saw the chocolate put into a box (either the left or right middle one). After an interval of 2 days, the subject was confronted with the original figure, this time, however, with the addition of another box placed at the side of the 'correct' box, thus completely changing the configuration (see Fig. 2).

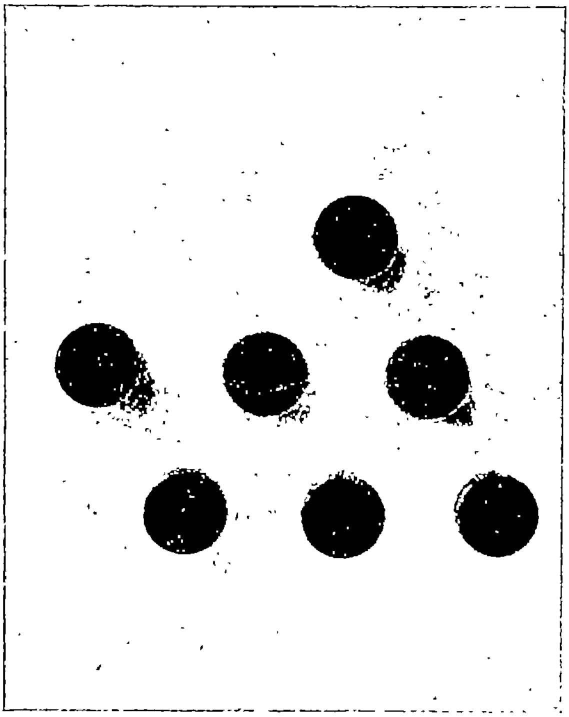

Fig. 2.

This test proved harder. Out of 44 subjects:

23 made correct choices, 21 incorrect.

The large majority of the incorrect responses consisted in the selection of the added box, and usually no surprise was evinced. In the correct responses, however, it was not unusual to see the subject pause for a moment before choosing the original box, taking some time to disentangle the configurations.

A smaller number of subjects was tried with the triangle of six boxes, without any addition being made:

After 14 days interval 11 were correct.
4 were incorrect.

This test seemed the most fruitful of results and was tried out among the low-grade boys. In this case the interval was greatly reduced, averaging 35 minutes. Seventy-six subjects were tried with the triangle of six boxes, one being added on the second exposure. The results were:

No response 12.
Incorrect box chosen 47.
Correct box 17.

The percentage of successes thus fell to 22 per cent. as compared with 50 per cent. in the first class (high-grade girls).

Among the large number of incorrect choices when the additional box was used, the added box itself was not so consistently selected as was the case with the high-grade girls, perhaps because in these latter some configuration was retained, while in the low-grade cases the responses seemed to be more in the nature of random guessing, though the guesses were evidently guided by a localisation--to the left or right in the field of vision.

Twenty-two, chiefly of the lowest grade, were tried without the addition of a box:

No response 8.
Incorrect 12.
Correct 2.

The 'no response' were all idiots who did not understand what was required of them, and were unable to point to the box after their attention was deflected for even half a minute. The majority of these were able to take the chocolate when the lid of the box was off, but when out of sight the chocolate seemed to be out of mind.

The fact that relatively small numbers of subjects were used in these experiments was due to the necessity of not repeating a test several times with the same person, and thus introducing a practice element into the results.

The conclusions from these preliminary experiments were that:

(1) The high-grade mental defective has considerable difficulty in recognizing a simple configuration when occurring as part of another configuration. By 'recognizing' is meant "the ability to know something as identical with something known previously." The new figure, differing only by an added box from the old one, yet tends to swamp the latter,

and prevents its recognition. It seems that in this form of test lies the greatest interest for future investigation. The language factor, the use of verbal instead of spatial configurations, and the impossibility of preventing the subjects from discussing the test with each other, interfered very seriously with the value of the test in the higher grade cases.

(2) In the low-grade defective, great difficulty is experienced in the discrimination of the parts of a relatively simple whole. This is probably due to the inadequacy of the revival of the memory idea, and not necessarily to the type of configuration in which the unit parts occur, and it would again appear to point to the great difficulty in spatial manipulation already noticed, and in any sort of analysis or even of retention of the original perceptual configuration.

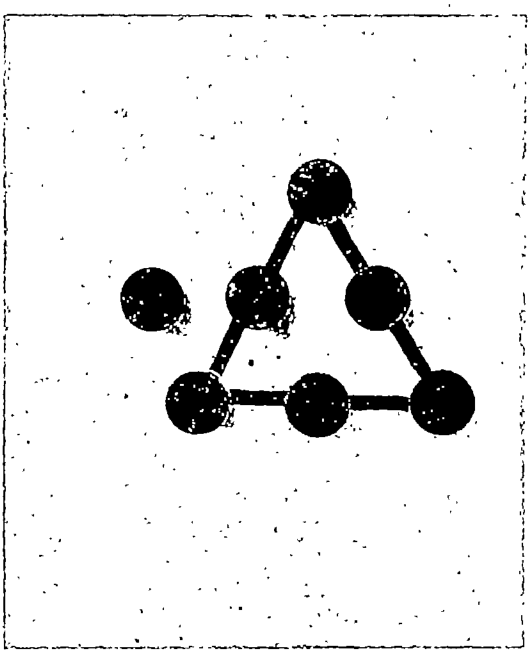

Fig. 3.

Thus, in the series of experiments with the triangle of boxes with the added box shown at the second presentation, 28 per cent. chose the added box, *i.e.* chose outside the original configuration, 72 per cent. chose from within the original configuration. Ignoring for the moment whether the choice within the original configuration (the triangle) was correct or not, one may ask wherein lay the difficulty in a choice which for the normal subject presents little difficulty. In fact the original triangular arrangement 'stands out' in such a way as to make the observer consider the added box as an obvious outsider. Is it because in the process of organisation of the given environmental data, the phenomenal configuration has inferior cohesion, inferior 'hanging together' in the case of the imbecile? To throw light on this point, those 16 boys who in the previous experiment had chosen the outsider, the added box, were retested (about 2 months later) in the following manner:

The initial presentation of the triangle of boxes was the same as before. After a similar interval of time the subjects were confronted with the configuration shown in Fig. 3, that is, with the added box as before, but with the original figure 'strengthened' by means of black strips about 1 cm. wide, emphasizing its triangularity beyond question. If, now, the original configuration existed in a form comparable to the normal, the reproduction of it by similarity with this second figure must be facilitated, and one would expect the choices to fall within the original figure, that is within the triangle of boxes.

Only four boys chose the added box this time, in spite of the fact that their choice before might be considered to have been 'stamped in' by success in the old experiment. Of the remaining 12, nine were correct. That is, employing a 'strengthened' figure in the second presentation, 75 per cent. were led back to the old figure.

This new experiment with the strengthened figure presented the second time with added box, the old unstrengthened triangular figure of six boxes having been shown the first time as before, was tried out among 80 fresh subjects. These were imbeciles, somewhat older than the previous group but equally low grade. They were in a totally different part of the colony, in one of the outlying establishments and therefore unfamiliar with the experiment. The result was as follows:

Choices inside the original configuration 49 (25 correct)	61 per cent.
Choices outside the original configuration, *i.e.* (the added box) (31 correct)	39 per cent.

Compare this with the old experiment where no strengthened figure was employed:

Choices inside original configuration	72 per cent.
Choices outside original configuration	28 per cent.

One sees, therefore, that in the theoretically 'easier' task, even fewer chose the original figure, and that at any rate the 'strengthening' did not facilitate choice within the original figure. It therefore seems as though the original environmental stimuli (the six boxes) are not organized as phenomenal configurations in the sense that they exist as a triangle, and that its 'parts' (post-analytically conceived by us) do not 'hang together' with sufficient cohesion to constitute an adequate configuration, adequate, that is to say, for the purposes of response.

Only a rough localizing ability was manifested, which may be conceived as a vague bearing to the left, possibly 'the left middle one,' the new figure (old configuration plus added box) being accepted without challenge, the relationship of difference not being educed between the new figure as perceived and the old figure as reproduced.

If we may formulate a tentative conclusion from such admittedly slender experimental evidence, one might say that in the mental defective the environmental configurations are not adequately organized into 'mental' or phenomenal configurations, or are not apprehended as such, that the sense of 'good' or 'definite figure' is reduced, and that in consequence the behaviour of the individual is not adapted to the environmental stimuli in a normal fashion.

(2) *Speech tests.*

Of the 80 fresh subjects, 75 who were available were subsequently tried out with a simple speech test to discover how they dealt with perceptual and simple conceptual configurations involving word symbolisation. Six pictures were used as follows:

(1) Dog and puppy.
(2) Cow and calf.
(3) Cat and kitten.
(4) Hen and chickens.
(5) Sheep and lamb.
(6) Donkey and foal.

Each subject was asked for each picture, "what is that?" The answers dog, cow, etc., were counted as correct, the mention of puppy, calf, etc., not being required. Then with all the pictures spread out in front of him, he was asked to find a dog, a cow, a cat, etc. These seemed to be a test of his retention and manipulation of very simple and familiar perceptual configurations symbolized by words. Sixteen of the 75 failed entirely to pass this test or got less than one-third correct. The others managed with little difficulty. The donkey was not always familiar and was sometimes called a horse and sometimes a nannygoat.

The third test was to present each picture and ask the subject "what does this do?"

24 out of 75 entirely failed to answer this.
11 gave a 'good' response and 5 a 'fair' response.

An example of a 'good' response is as follows:

The dog—eats bones.
The cow—tosses you up.
The cat—catches mouses.
The hen—lays chickens.
The sheep—eats grass.
The donkey—draws a cart.

An example of a 'fair' result is as follows:

Dog—bites.
Cow—kick you.
Cat—bite you.
Hen—lays eggs.
Sheep—washing it.
Donkey—kick you.

In this it will be observed that the responses are reasonably good, but the response to sheep is simply descriptive of what is depicted in the picture, while there is a definite perseverative tendency, dog and cat both bite, and cow and donkey both kick.

In three cases the response was in the form of noises only, *e.g.*

Dog—barks.
Cow—moo.
Cat—mi-ows.
Hen (described as cuckoo)—makes a cuckoo noise.
Sheep—makes a funny noise.
Donkey—makes a funny noise.

Ten responses were in terms of description of what was depicted in the picture, *e.g.*

Dog—big one looks at little one.
Cow—puts head over calf.
Cat—run after big kitten.
Hen—lays little ones and eggs.
Sheep—little one looks up.
Donkey—puts head over the other.

The remaining 22 showed an irresistible perseverative tendency, and the form of this was influenced by the perhaps unfortunate fact, that there was a young animal in each picture, *e.g.*

Dog—lays little dogs.
Cow—lays calves.
Cat—lays kittens.
Hen—makes young one.
Sheep—lays lambs.
Donkey—lays donkey.

This 'lays' response was very common. Another type may be exemplified by the following:

Dog—eats bones.
Cow—eats grass, walks about.
Cat—walks about.
Hen—walks.
Sheep—walks about.
Donkey—eats grass, walks about.

A few when pressed by asking "does this do anything else?" would give such responses as cow—gives milk, but this was rare.

From this test it is clear that just as with the spatial configurations, so was it with the configurations symbolized by words. Directly the configurations got in the least complex, these low-grade cases could not grasp them at all, or only with great difficulty.

The marked tendency to perseveration suggests that tests for Spearman's '*C*' factor (9) would be interesting in mental defectives, and it is hoped to carry these out later.

The behaviour of defectives in respect of configurations, whatever the nature of these may be, seems to be closely correlated, and the power of manipulating configurations is apparently an important function of the brain.

If it is true, as is generally accepted, that mental deficiency is due to a paucity of neurones in the brain, and that mind is a function of brain, then perceptual and conceptual configurations must be a function of more or less complex patterns of neurones and the general conception of the neurogram (Morton Prince (10)) or engram (Semon (11)) is upheld.

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INTELLIGENCE AND FERTILITY[1].

By SHEPHERD DAWSON.

I. *Previous enquiries* (pp. 42–44).
II. *Scope of the enquiry* (pp. 44–45).
III. *Results* (pp. 45–47).
IV. *Comparison of results with those of other enquiries* (pp. 47–49).
V. *Discussion and conclusions* (pp. 49–51).

I. Previous enquiries.

The investigation of the relationship between intellect and fertility is beset by peculiar difficulties, chief among which are the exact measurement of intellect, the collection of reliable information regarding birth-rates and the avoidance of the danger of reasoning from exceptional cases. The aim of this paper is to give the results of some observations on the birth-rates of people of different grades of intelligence and to show their significance.

The problem is not a new one. Sir Francis Galton (7) went into the histories of 180 Englishmen of scientific distinction, and found that one in every three of them had no children, that those who did have children had fewer than their own parents, and that, when childless marriages were excluded and those in which children might yet be born, the average number of living children in their families was 4·7, whereas the average size of the families from which they sprang, excluding for the most part children who died in infancy, was 6·30. Havelock Ellis (6) found that the average family of 214 fertile marriages of British men of distinction consisted of 5·45 children. Cattell (4) investigated the sizes of the families of 440 American men of science, and found that the average number of children in them was 2·3: he included barren marriages. These observations have suggested that fertility varies inversely with intellect, a suggestion which is supported by popular opinion. On the other hand, it is a common belief among those whose expert knowledge qualifies them to give an opinion that, at the other end of the scale, mental defectives are relatively less productive than the average.

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It is possible that abnormality of intellect either in excess or in defect may go with lessened fertility. The only satisfactory procedure is to consider a group which is representative of the population as a whole, or approximately so, not those who have been selected because of their imbecility or their scientific distinction.

It is also necessary to have some reliable method of estimating intelligence. Perhaps the most unreliable method is that of Dr Warner (12), who between 1888 and 1894 in London investigated the relationship between dullness and physical condition in 100,000 children by lining them up in ranks and picking out the dull ones by inspection. It is a popular method with which all can sympathize, for we all claim peculiar insight into the diagnosis of intellect (we may be unable to guess a man's politics by looking at him, but we have no hesitation is assessing his intelligence), and the less we test our diagnoses, the more vigorously are these claims made. Warner, it should be mentioned, worked before the days of mental testing.

Prof. Karl Pearson (9) and Dr Heron (8) improved on this method by using the estimates made by teachers under prescribed conditions and by testing the consistency of these estimates: but even teachers are fallible, and they are apt to judge a child's intelligence solely or mainly by the progress he makes in his school work.

Intelligence tests have put the matter on a different footing, for they give a measure of intelligence which is superior to any based on personal opinion, in that it is objective, impersonal and numerical. Group intelligence tests have been applied to school children and the results correlated with the number in the family. Bradford (2), using the Otis Intelligence Tests on 393 boys and girls of age 10, found between size of family and test score a correlation of $-0{\cdot}25 \pm 0{\cdot}03$. Sutherland and Thomson (10) tested 1924 children of ages $10\frac{1}{2}$–$11\frac{1}{2}$ in the Elementary Schools of the Isle of Wight, 386 Grammar School boys (ages $10\frac{2}{3}$–$17\frac{1}{3}$) in Newcastle-on-Tyne, 395 boys and girls (ages $7\frac{1}{2}$–$15\frac{1}{12}$) in the Moray House School, Edinburgh, and 30 Grammar School boys (ages $10\frac{5}{12}$–$12\frac{1}{12}$). They found correlations of $-0{\cdot}154 \pm 0{\cdot}023$ and $0{\cdot}218 \pm 0{\cdot}019$ between the intelligence and the size of the families of the Isle of Wight children. Selecting the children of 371 families which seemed to be complete, they obtained a correlation of $-0{\cdot}177 \pm 0{\cdot}04$. Their other groups gave no significant correlation, which they suggested was probably due to the fact that most of the children had been selected for their intelligence: the families to which they belonged were smaller than those of the first group. Chapman and Wiggins (5) tested 650 American children and found a correlation of $-0{\cdot}33 \pm 0{\cdot}024$ between intelligence and size of family.

Group tests, however, have been looked on with some suspicion, for they are largely scholastic and they measure intelligence in bulk, as it were. My apology for presenting this paper is that in Glasgow we have, in the course of other enquiries, applied *individual* Binet tests to over a thousand children and have fairly accurate information regarding the families from which these children come.

II. Scope of the enquiry.

In all the investigations that have been made into the connection between intellect and birth-rate the general plan of procedure has been the same: some estimate of the intelligence of a child has been made and this has been correlated with the size of the family to which he belongs. In other words, the fertility of his parents has been correlated with the intelligence, not of the parents, but of the child himself. This is a defect which will remain until really satisfactory tests for adults have been devised, or until those now in use have been proved to be reliable, or until the testing of children has become so general that a fair sample of the adult population will have records of tests which were applied to themselves in their childhood. It is a defect which our enquiry shares with the others.

The children whom we examined were not quite representative of the whole population: socially, they were below the average; about a third of them were sons and daughters of labourers; the fathers of about a fifth of them were unemployed or had no apparent occupation: those of the rest were steel-workers, boiler-makers, machinists, sailors, firemen, porters, drivers, miners, and small tradesmen. Many of the children came from overcrowded homes. Intellectually, they were on the whole below the average: their mean intelligence ratio was 91, while that of the whole population is about 100. In the whole population it has been estimated by Burt (3) that 15 per cent. have intelligence ratios above 114, 70 per cent. between 85 and 114, and 15 per cent. below 85: in the children we examined the proportions were 4·5 per cent. above 114, 63·4 per cent. between 114 and 85, and 32·1 per cent. below 85.

There were 1239 of them; so that numerically they represented fairly well the class to which they belonged. Their ages varied from 3 to 14 years, with an average of 7·7.

As regards the family to which a child belonged, there was a record of the number of children alive and the number of fatalities (ante-natal,

natal and post-natal), the ages of the children and the parents, and the number of years the latter had been married. The ages of the mothers varied from 20 upwards, with an average of 35. These records are, of course, subject to the errors that attend all records given verbally, but there is no reason to believe that such errors are sufficiently numerous seriously to affect the accuracy of our generalizations.

Many of the families were not complete, which creates a difficulty when we try to correlate intelligence with size of family. There are several ways of dealing with this difficulty. (1) We can neglect the difference between completed and uncompleted families, assuming that each kind is fairly evenly distributed over the whole and that the inclusion of the latter will merely tend to obscure slightly the correlation. This seems legitimate when the aim is, as mine is in this paper, merely to find the comparative rate at which the different grades of intelligence multiply. It will, however, give only approximately the average size of the completed families. Again, (2) we may consider the age of the child and the number and ages of his brothers and sisters and decide from these whether the family is probably completed. Or (3) we may assume that a family is complete when the mother is over 45 years of age. Or, finally, (4) we may consider the rate at which the children have been born. Each of these methods when used alone gives only part of the information that ought to be considered. I propose to use here only the first and the third.

III. Results.

Table I.

	Correlation coefficients			
	All families		Completed families	
	N	r	N	r
I.R. and children alive	1239	−0·12 ±0·02	140	−0·10 ±0·02
I.R. and No. born alive	1239	−0·19 ±0·02	140	−0·26 ±0·02
I.R. and No. of births	1239	−0·20 ±0·02	140	−0·30 ±0·02
I.R. and No. of possible lives	1239	−0·20 ±0·02	140	−0·28 ±0·02

Table I shows the correlation between the intelligence ratios (I.R.) of the children and (*a*) the number of children alive in the families to which they belonged, (*b*) the number born alive, (*c*) the number of births (live or dead), and (*d*) the total number of possible lives. The correlations are *small*, which means that there is a slight relationship, but very slight, between the intelligence of the children and the size of the families to which they belonged. They are all *negative*, which means that dull children on the whole come from the bigger families, or, if we assume, as is commonly done, that on the whole the intelligence of children is like that of their parents, it means that dull parents on the whole have slightly more children than brighter parents. The correlation increases as you go down the Table. This is because the fatalities are relatively greater among the duller families. We shall return again to this point.

If we pick out those families in which the mother was 45 years old or over when the child was tested (and the family probably complete), we get the figures in the last two columns of the table. The number of families here is small (only 140), but the results are similar: the correlations are of the same order and there is the same increase in their value as you go down the table, but it is not quite so regular. The magnitudes of the correlations in the last columns are very like that found by Terman (11) in his study of the histories of some 643 children with I.R.s varying from 130 to 200. In 92 of the families to which these gifted children belonged the mother was 45 or more years of age: in these the correlation between the I.R. of the child and the size of the family to which he belonged was – 0·271 ± 0·062.

As correlation coefficients are to many people fearsome things which are regarded with some suspicion, I propose to bring out their significance in another way, one which is less acceptable to the statistician, but more intelligible to the rest of us. I have divided the children into three groups, bright, average and dull, and in making this classification I have considered those with I.R.s of 115 or more as bright, 85 to 114 as average, and 84 or less as dull.

Table II. *Showing average number of births, etc., of the 'bright,' 'average' and 'dull.'*

Intelligence ratios ...	Over 114	114–85	Below 85	All
No. of families ...	56	785	398	1239
Deaths, ante-natal (D_a)	0·29 ± 0·06	0·33 ± 0·02	0·44 ± 0·03	0·36 ± 0·15
,, natal (D_n)	0·09 ± 0·04	0·14 ± 0·01	0·16 ± 0·02	0·14 ± 0·01
,, post-natal (D_p)	0·64 ± 0·10	1·00 ± 0·03	1·32 ± 0·06	1·09 ± 0·03
Survivors (A)	3·41 ± 0·16	4·43 ± 0·05	4·73 ± 0·08	4·47 ± 0·04
D_a+D_n	0·38 ± 0·05	0·47 ± 0·02	0·63 ± 0·03	0·52 ± 0·02
$D_a+D_n+D_p$	1·02 ± 0·12	1·47 ± 0·04	1·92 ± 0·07	1·59 ± 0·03
D_p+A	4·05 ± 0·20	5·43 ± 0·06	6·05 ± 0·11	5·56 ± 0·06
D_n+D_p+A	4·14 ± 0·21	5·57 ± 0·07	6·21 ± 0·11	5·70 ± 0·06
$D_a+D_n+D_p+A$	4·43 ± 0·21	5·90 ± 0·07	6·65 ± 0·12	6·06 ± 0·06

NOTE. The value which follows the sign ± is the 'probable error' of the mean.

Table II gives the result of this classification: it shows the number of families in each of these groups, the average number of fatalities (ante-natal, natal and post-natal), the average number of survivors, and various combinations of these. In every row the average increases as you pass from the bright to the dull group. The number of possible lives ($Da + Dn + Dp + A$) is about 50 per cent. greater in the dull group than in the bright; so also is the number of births. This looks very serious; but there are relatively more fatalities in the dull group (perhaps because the more intelligent parents are better able to care for their children). This reduces the discrepancy and accounts for the fact that the correlation between the I.R. and the number of survivors is less than that between the I.R. and the number of births. It does not, however, remove this discrepancy: the dullest have more survivors (but not many more) than the average, and the bright have fewest of all.

Table III, which gives the corresponding values for children whose mothers were 45 or over at the time of the testing, points in the same direction; indeed, the differences are greater here. The actual averages are greater, as one would expect, for the average age of the mothers in the whole population was about 10 years less; the average numbers of survivors in the three groups are 3·9, 6·0 and 6·7, as against 3·2, 4·4 and 4·7 in Table II. I do not, however, wish to attach much importance to these figures, for the total number of families represented is not great, being only 140. They do, however, corroborate the results given by the larger group.

Table III. *Showing the average number of births, etc., in* 140 *completed families.*

Intelligence ratios ...	Above 114	114–85	Below 85	All
No. of families ...	7	88	45	140
Deaths, ante-natal (D_a)	1·0	0·6	0·8	0·7
„ natal (D_n)	0	0·2	0·2	0·2
„ post-natal (D_p)	0·7	1·7	2·6	2·0
Survivors (A)	3·8 ± 0·37	6·0 ± 0·16	6·7 ± 0·28	6·1 ± 0·14
D_a+D_n	1·0	0·8	1·0	0·9
$D_a+D_n+D_p$	1·7	2·5	3·6	2·9
D_p+A	4·6 ± 0·64	7·7 ± 0·20	9·3 ± 0·28	8·1 ± 0·18
D_n+D_p+A	4·6 ± 0·64	7·9 ± 0·21	9·5 ± 0·35	8·3 ± 0·18
$D_a+D_n+D_p+A$	5·6 ± 0·58	8·5 ± 0·23	10·3 ± 0·37	9·0 ± 0·20

IV. COMPARISON OF RESULTS WITH THOSE OF OTHER ENQUIRIES.

How far are these facts representative of what is happening in the population? It has been remarked that the children we examined are not quite representative of the whole population, that they are slightly below average. There is some ground for thinking that in a group which contained a higher proportion of brighter intellects the difference between the dull and the bright in respect of size of family would be greater.

An enquiry into "Mortality rates in Glasgow families" (1) was made by Miss Agnew about nine years ago. She analysed information regarding 1789 poor families and 455 that were well-to-do: the poor families were like those we examined and the well-to-do were patients of a consulting pediatrist. She found that when only families of more than one child were included, the average number of possible lives in the 1789 poor families was 5·6, whereas in the well-to-do it was 3·2. The corresponding figure for the population we examined is 6·0, which is not far from 5·6. It is hardly necessary to remark that 'well-to-do' and 'very intelligent' are not synonymous terms, that it would be foolish to suppose that all these 455 well-to-do families were of superior intellect, for the size of one's banking account does not always vary directly with the degree of one's intelligence: but there is good reason for thinking that there is a relationship between them: Burt, Terman and others have shown that there is an appreciable correlation between intelligence and social position.

There is another piece of evidence that points in the same direction, namely, the results of a comparison of the records of other investigators. It is somewhat difficult to make this comparison on account of differences in the mode of selection of the families studied. Table IV summarizes the facts.

Table IV.

	N			Av. No. of live births
Galton	180	*C*	Distinguished English men of science	4·7
Ellis	214	*C*	British men of distinction	5·4
Sutherland and Thomson	1924	*I*	Isle of Wight Elementary School children, average I.R. not over 100	4·3
Sutherland and Thomson	395	*I*	Edinburgh College School, average I.R. high	3·3
Sutherland and Thomson	386	*I*	Grammar School boys, average I.R. 112·5	3·1
Terman	92	*C*	American children, I.R. 130–200	3·4
Dawson	56	*I*	Live births, I.R. 115–139	4·1
,,	785	*I*	,, 85–114	5·4
,,	398	*I*	,, below 85	6·0
,,	7	*C*	,, above 114	4·6
,,	88	*C*	,, 85–114	7·7
,,	45	*C*	,, below 85	9·3

N = No. of families.
C = ,, completed families.
I = ,, families not all of which were complete.

In interpreting this table it must be remembered that there has been a steady decrease in the birth-rate, and that the basis of selection of the groups studied has been social and scientific success rather than intellectual endowment. Galton's average (4·7 for completed families of

Englishmen who distinguished themselves in science) is nearly the same as the average in our brightest group of completed families (4·6). Terman's figure for his exceptionally gifted children (3·4) is nearly the same as Sutherland and Thomson's for Grammar School boys and College children (3·2); but Terman's families were complete, while the others were not; and this figure, again, is like that of Miss Agnew for 'well-to-do' Glasgow families (3·2).

If sterile marriages also were included (about which we have no information), the difference between the dull and the bright in respect of size of family might be still greater, for sterility seems to be common among the bright. Our figures, therefore, probably underestimate the difference in fertility of the various levels of intelligence.

V. DISCUSSION AND CONCLUSIONS.

One is tempted to conclude that the less intelligent elements of the population are increasing at a greater rate than the more intelligent, and that in time they may vastly outnumber them, for it must be remembered that the influence of a differential birth-rate would be cumulative. A difference of *one* in the average surviving families of the dull and the bright may seem a small matter, but nationally it may be serious. This can be demonstrated by a simple arithmetical speculation—and it must be remembered that it *is* a speculation. We know that 3·2, 4·4 and 4·7 are the average numbers of living children in our bright, average and dull groups. We do *not* know how many of these will reach maturity and establish families of their own: the averages will be less than these. Let us suppose that it is 3 in the bright group and 4 in each of the others. Suppose that the groups do not intermarry (which certainly is only approximately correct), and that the average fertility of each group remains the same from one generation to another. Then the 4th filial generation of the "bright" group would be $(1{\cdot}5)^4$ or 5 times as big as the parental, while the others would be $(2)^4$ or 16 times as big; the 10th filial generations would be 57 and 1024 times as numerous as the parental. As remarked above, it has been estimated that the percentages of the present total population with I.R.s above 114, between 85 and 114, and below 85 are 15, 70 and 15 respectively; therefore, in the 10th filial generation the numbers in these groups would be in the proportions 15 (57), 70 (1024) and 15 (1024), or 1, 84 and 18, instead of 1, 4·7 and 1, as at present.

This looks very alarming, for it suggests that a couple of centuries hence superior intellects may be *relatively* much scarcer than they are to-day. Let me repeat that this calculation is not a forecast; it has been made merely to show the *tendency* of the effect of a cumulative differential birth-rate. The conditions are too complex and our ignorance of the facts too great to permit anything more than that. We know that in this country the birth-rate has been steadily going down and that the population is approximately stationary. We know that the different grades of intelligence *do* intermarry, that clever men sometimes have stupid wives, and clever wives stupid husbands; but we do not know the extent to which this happens. Nor do we know whether the fertility of each group remains constant. It is well known that superior intellects sometimes spring from apparently mediocre stock, but we still require exact information regarding the extent to which they pass on their brilliance to their offspring. The common belief is that able parents have on the whole gifted children, that mental capacity is inherited like stature, eye colour, and other physical characters. No one who has studied the subject at first hand, in the hospitals and the schools, can resist the conviction that mental defect and dullness of intellect may be due to any one of several causes; they may be due to pathological conditions—minds that have been normal may as a result of disease or accident (*e.g.* an attack of encephalitis lethargica) deteriorate even to a state of imbecility, or they may be due to a failure to develop normally (an agenesia), or they may be the result of traits inherent in the stock that produced them.

We are very much in need of methods of distinguishing these cases with some approximation to accuracy. We are still more in need of studies of mental inheritance akin to those which have been made on the inheritance of physical characters in the lower animals: we do not yet know whether natural dullness is one genetic character or many, nor do we know what kind of character (or characters) it is.

In conclusion, allowing for our ignorance regarding the number of childless marriages and the number who do not marry, our observations have shown that the duller elements of society have more children than the more intellectual, that, although their casualties are higher, they *may* be leaving more survivors, and they suggest the gloomy possibility that dullness is being bred rather than intellect, and that some of the schemes which have been put into operation to promote the welfare and happiness of the state may be doing something to preserve and multiply the dull, and so accentuating our social problems. In view of the tendency of the state and private enterprise to do so much for the unfit, it is obvious that this question of the relationship between intellect and birth-rate is of more than academic importance and calls for a careful study, not only of the facts regarding birth-rate, but also of survival and of the laws of mental inheritance, for, until these are known, speculation on methods of treatment is somewhat futile.

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A NEW PERFORMANCE TEST OF INTELLIGENCE.

By W. P. ALEXANDER.

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I. *Introduction* (pp. 52–53).
II. *Name* (p. 53).
III. *Description of the test* (pp. 53–54).
IV. *Test administration* (pp. 54–58).
V. *Results* (pp. 58–61).
VI. *Summary* (p. 61).
Appendix (pp. 61–63).

I. Introduction.

There is probably no branch of psychology in which greater progress has been made in recent years than in intelligence testing, yet it is still true that we are in need of new performance tests. Most of the performance tests so far devised fail at the higher age levels. The Pintner-Paterson scale (1) is of little service above the age of say ten years. In 1925 Gaw (2) tried a wide variety of performance tests in London and reports that "There are very few performance tests (either parts of scales or single tests) sufficiently difficult to be used as a basis for comparing normal adults." Later Collins and Drever (3), realizing the need for variety of tests, standardized their performance scale. This scale is a marked advance on those preceding it, but it by no means solves the problem. As late as 1929 Earle (4) is led to the conclusion that there are few performance tests of much value at school-leaving age. It is in the hope that the test to be described may be helpful in this situation that it is presented.

The tests which were used in the investigation were as follows:

(1) A new test (performance).
(2) The Collins-Drever Scale (performance).
(3) The National Group Scale, form 2 A (verbal).

The criterion of the value of the new test which is used is the agreement with the results of the other tests. This raises the problem whether we can compare results in performance tests with results in verbal tests.

It is not intended to deal with this problem in this article, though it is hoped to deal with it later. For the purposes of the present discussion we assume that all the tests are measures of general intelligence. We accept the point of view of Spearman (5) and assume the tests are all measures of "*g*." The results are then comparable. The two scales we have selected for comparison are recognized as among the best of their kind, so we have a fairly sound criterion of value for the new test.

II. NAME.

The name to be given to the new test is "The Passalong Test." This at once indicates the nature of the test and avoids the use of any term in testing which might be misleading. The name is borrowed from a puzzle which appeared while this investigation was in progress. This puzzle is of such difficulty that though it embodies the same idea as the test there will be no decrease in the value of the test. Both are taken from a puzzle of such complexity that even the most superior adults failed to do it successfully at all. I mention this because the criticism of the material being in popular use could be made of several tests produced in late years. In this case the difference in the order of difficulty makes the criticism without validity.

III. DESCRIPTION OF THE TEST.

The test comprises nine boxes each containing a number of small blocks, one of which (in two cases two) is coloured red while the others are coloured blue. One end of the box is red and the other blue, and the problem in each case is to move the red block to the red end of the box without lifting the blocks out of the box but by moving them within the limited space left in the box. The blocks have all a definite size relationship and the combination of blocks varies in each box. As a result the problem varies in difficulty and the aim has been to make the complexity of the task increase as we go through the series. The first box presents a problem which is simple for the average five year old, while the last box is sufficiently difficult to test superior adult intelligence. The boxes have been so arranged that each is a preparation for the next. In this way the test assesses the subject's ability to profit by experience as well as measuring the insight with which he reacts to a novel situation. We give below a diagram showing the initial positions for the nine boxes. The first box (Fig. 1) serves to introduce the subject to the material and acts as a shock absorber. It comprises three blocks fifteen-sixteenths of an inch square and half an inch thick, in a box of which the inside measurements are two inches by two inches by three-eighths of an inch. The blocks are thus projecting above the edge of the box and can easily be moved. The arrangement of the blocks in the box is shown in Fig. 1. The usual colour convention is followed, blue being represented by horizontal lines and red by vertical lines. The other boxes are shown in Figs. 2–9. In each case the diagrams are in correct proportion, so there is no need to give details of dimensions. The base of the box is in every case half an inch thick and the sides of the box three-sixteenths of an inch thick. The blocks are always half an inch thick and the box three-eighths of an inch deep.

The increase in difficulty is shown in the diagrams. Box 7 was introduced as a habit breaker, but it would appear to serve no useful purpose and it is therefore proposed to omit it in future work. There appears to be too great a step from box 8 to box 9, and it is therefore intended to introduce a new box between these two (see Appendix). The end positions are easily obtained from the diagrams showing the initial positions. In boxes 1, 2, 4, 5, 6, 8, 9 the end position is got by turning the figure upside down and calling the red end blue and the blue end red. The end position for box 3 is the same as that for box 2. The end position for box 7 is got by interchanging the red block with the blue one exactly opposite. In the administration of the test an exact picture of the end position of each box is placed before the subject so that instructions are reduced to a minimum. The pictures are full size and coloured in the same way as the test material, so that the subject has a clearly defined goal towards which he has to strive.

IV. TEST ADMINISTRATION.

In this initial investigation we were concerned to try all possible methods of scoring so as to ascertain which was likely to yield the best results. As a consequence we have a more involved method of administration than would be desirable if the test were to form one of a scale. As it is intended that the test should be used as a component in a scale of as wide variety as possible we give in an Appendix a method of administration and scoring which will reduce the time required for the test. This is the method we propose to adopt in further work.

As we are anxious to show the value of the test from the results so far secured, we give here the method of administration and scoring used in the present investigation. After the usual precautions to ensure that *rapport* has been established between the examiner and the subject, box 1 is placed before the subject along with picture 1. The examiner explains that the blocks can move and demonstrates by moving the red block from side to side. After warning the subject that the blocks must not be lifted out of the box he tells the subject to move the red block to the red end of the box so that it will be like the picture. The procedure with subsequent boxes is simply to place the box and the picture before the subject and say, "Move the red block to the red end so that it will be like the picture." Care should be taken that the subject is quite clear as to what is to be done. For example in box 9 it is permissible to draw the attention of the subject to the fact that the small blocks are inside and must be inside at the finish as in the picture.

With deaf children the first box should be demonstrated and the subject signed to repeat. With subsequent boxes the examiner points to the box and then to the picture and motions the subject to proceed. I have only tested one deaf case so far and found this method quite satisfactory.

Time each performance with a stop-watch and in addition note the number of moves required. A move is a change of position of a block. If two blocks are moved with one motion this counts one move. If a block is moved to a new position and then returned to its original position this counts two moves. Where the subject is merely fingering a block while thinking no moves are counted.

Table I. *Showing the time limit for each box, and the time allotted to each box for perfect performance.*

Box	Time limit (min.)	Time allotted (sec.)
1	3	10
2	3	15
3	3	20
4	3	30
5	3	30
6	3	30
7	3	15
8	3	30
9	3	45

For each box there is an allotted time, and if this time is exceeded the box is demonstrated and the subject then asked to repeat. This is to ensure that all the boxes are done successfully by all the subjects. Each box is repeated till success is achieved in less than a given time (see Table I). This is to ensure that insight has been gained into the problem. These times make no demands on motor co-ordination. As has been already said, this very complete method was only adopted for the

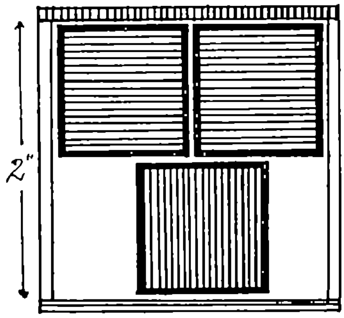

Fig. 1.

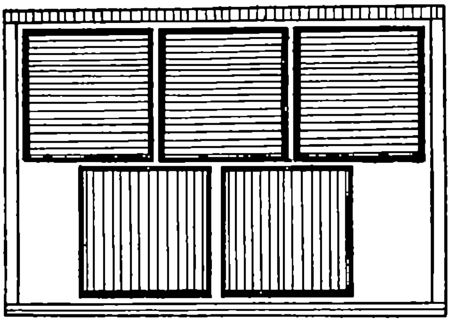

Fig. 2.

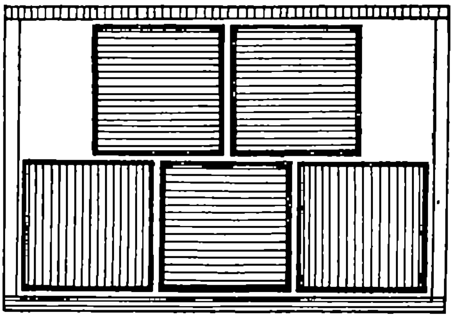

Fig. 3.

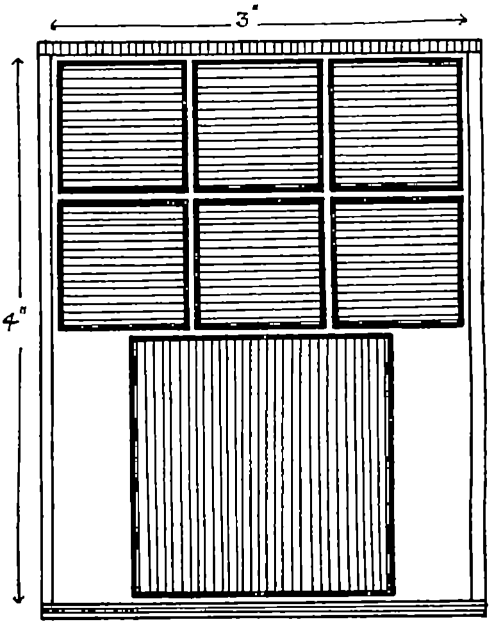

Fig. 4.

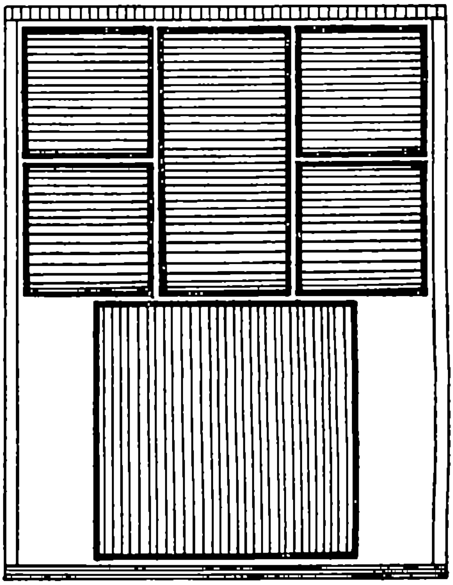

Fig. 5.

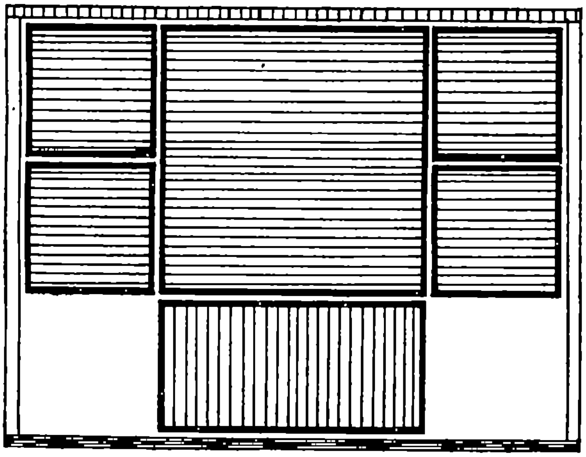

Fig. 6.

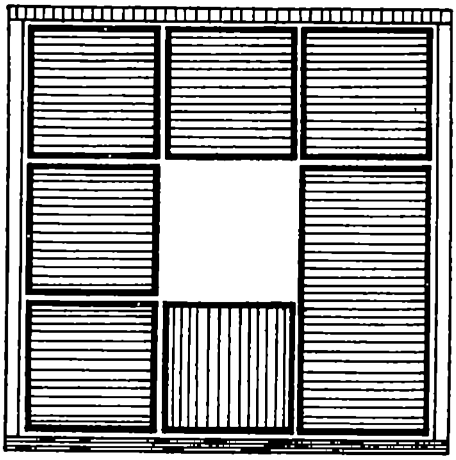

Fig. 7.

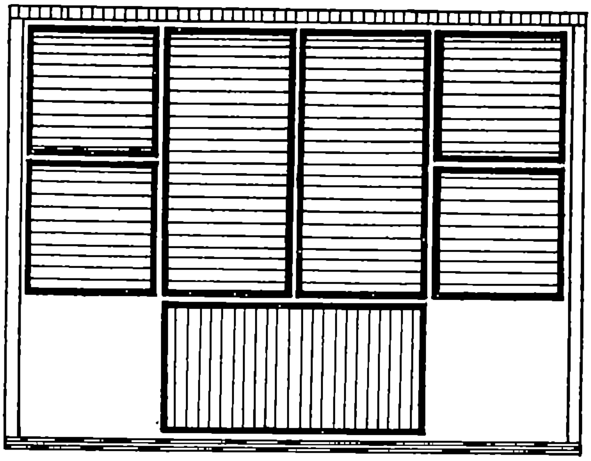

Fig. 8.

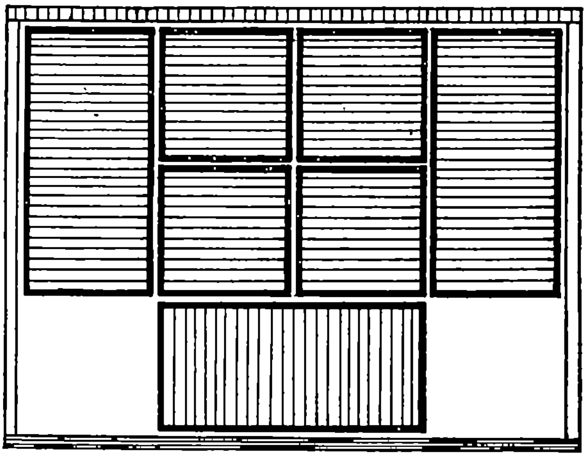

Fig. 9.

sake of trying all possible methods of scoring. It causes the total time required for the test to be longer than is desirable (total time from 25 to 35 min.), hence the method suggested in the appendix. The scores noted are then, time in each trial, number of trials, number of moves in each trial and number of demonstrations needed.

We give in Table II an illustration of a scoring sheet. It should be mentioned that this subject was one of the most intelligent tested, having an I.Q. on the Terman revision of the Binet scale of 127, representing in his case a mental age of 15 years 7 months. It is worth noting his performance on box 9, since it provides evidence of the difficulty of this problem.

Table II. *Showing the method of scoring.*

Box	Time min.	sec.	Moves	Time sec.	Moves	Time min.	sec.	Moves
1		8	7					
2		25	14	14	12			
3		16	15					
4	2	20	54	20	19			
5		32	23	14	18			
6		25	26					
7		6	6					
8		28	24					
9	3	0	87	Demonstration		1	0	51
		40	27					
Totals	8	20	283	48	49	1	0	51

Total time ...	10 min. 8 sec.
Total moves ...	383
Total trials ...	14
Demonstrations ...	1

V. RESULTS.

Our first purpose here is to ascertain which method of scoring should be adopted. We tried four methods, viz. total time, total trials, total moves, and total moves with one hundred moves added for each demonstration. Notice that we could have similarly valued demonstrations in terms of trials or time, but as we had evidence that a new method of giving the test was desirable in which demonstrations would not be valued this was not done.

The correlations between the Passalong test and the other two scales are shown in Table III.

The value of a demonstration at one hundred moves was taken on the grounds that the average number of moves made in 3 min. was approximately this number. Table III shows that if no valuation is put on demonstrations then time is the best measure. For that reason time is the measure suggested in the appendix. The valuation of demonstrations however gives a better measure with the present method of administration, and it is used therefore in the results which are given here. The value of the test is indicated by the fact that all the methods of scoring give fairly high correlations.

Table III. *Showing the value of different methods of scoring the Passalong Test.*

	Passalong Test scored in			
	Time	Trials	Moves	Moves with demonstrations valued
National Scale	0·71	0·54	0·60	0·79
Collins-Drever Scale	0·69	0·63	0·56	0·77
Mean	0·70	0·58	0·58	0·78

Before giving a brief indication of the results obtained with the new test, it may be well to indicate the nature of the group of subjects tested. The group numbered 50 at the beginning, but was reduced to 45 before the completion of the work. The subjects were boys in an advanced division whose average age was 13 years 2 months. The median age was 13 years 6 months, the quartiles being 12 years 9 months and 13 years 9 months. The age of the group allows us to draw conclusions as to the value of the test at school-leaving age.

The test and the two scales were given to this group of subjects and the results for the scales as units are shown in Table IV.

Table IV. *Showing intercorrelations for the Passalong Test, the National Scale, and the Collins-Drever Scale.*

		A	*B*	*C*
Passalong Test	*A*	—	0·79	0·77
National Scale	*B*	0·79	—	0·86
Collins-Drever Scale	*C*	0·77	0·86	—

According to this table the Passalong Test is almost as good a measure of intelligence as the Collins-Drever Scale as a whole, if we take as the criterion correlation with the National Scale. Since the National Scale has a correlation of 0·80 with the Terman revision of the Binet Scale this is justified as a rough estimate. The Passalong Test may reasonably be expected to be less reliable than the Performance Scale, but as first evidence this leads us to expect the new test to prove of real value.

A better method of assessing the value of the new test is to compare it with the individual tests which comprise the Collins-Drever Scale. This is done in Table V, which gives intercorrelations for ten tests, the eight of the performance scale, the new test, and the National Scale retained as a unit.

By arranging the table in hierarchical order the tests are put in order of merit on the criterion of average intercorrelation with all the other tests, a very sound criterion since all are measures of intelligence.

Table V. *Showing intercorrelations for ten tests in hierarchical order*[1].

		A	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*I*	*J*	Mean
National Scale	*A*	—	0·75	0·75	0·72	0·62	0·56	0·54	0·41	0·45	0·26	0·562
Passalong Test	*B*	*0·75*	—	*0·76*	*0·74*	*0·53*	*0·59*	*0·42*	*0·34*	*0·22*	*0·23*	*0·510*
Cube Construction	*C*	0·75	0·76	—	0·65	0·53	0·45	0·38	0·43	0·35	0·27	0·509
Painted Cube Test	*D*	0·72	0·74	0·65	—	0·55	0·47	0·40	0·37	0·23	0·22	0·483
Picture Completion	*E*	0·62	0·53	0·53	0·55	—	0·24	0·26	0·13	0·34	0·32	0·391
Knox Cube Test	*F*	0·56	0·59	0·45	0·47	0·24	—	0·59	0·12	0·32	0·12	0·384
Domino Test	*G*	0·54	0·42	0·38	0·40	0·26	0·59	—	0·31	0·19	−0·03	0·340
Profile Test	*H*	0·41	0·34	0·43	0·37	0·13	0·12	0·31	—	0·35	0·33	0·310
Weights Test	*I*	0·45	0·22	0·35	0·23	0·34	0·32	0·19	0·35	—	−0·02	0·270
Form-board Test	*J*	0·26	0·33	0·27	0·22	0·32	0·12	−0·03	0·33	−0·02	—	0·190

When r is 0·76, P.E. is 0·042; when r is 0·02, P.E. is 0·101.
When r is 0·28 it exceeds 3 times its P.E.

Table V reveals several interesting facts. We note in the first place that the National Scale is first in order of merit, and when we remember that it is a composite of five tests we are not surprised at this result. In any case our concern here is chiefly with the performance tests, and we find an exceedingly interesting fact in this connection. The new test takes a higher place than any of the tests in the Collins-Drever Scale. This result is more than we had hoped for and warrants great hope that we have in the Passalong Test an exceptionally good test of intelligence at age level 14 years.

We may take a glance at the general trend of Table V to see whether it is in accord with the results of other investigations with performance tests. We find that this is so. For example, we find that the Cube Construction and Painted Cube Tests are alongside the Passalong Test as the best in the series. At the other end we find the form-board test the weakest of all. This is a similar result to that recorded in the National *Memoirs* (6) and also to that found by Earle (4). The Picture Completion Test is another which has been used in such investigations and again we find our placing of it in agreement with other results. Our table then is in accordance with the results of other similar investigations, and we may reasonably conclude that our finding regarding the new test has every likelihood of being a valid one.

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[1] No description of the tests comprising the Collins-Drever Scale is given here. The method and material used was exactly as in Collins and Drever's book (3).

I have deliberately made this discussion of results as brief as possible, merely trying to give some indication of the possible value of the new test. The purpose of this article is largely one of description. When the new test has been tried on a large number of cases we will be in a better position to estimate its true worth. Having only a small number of cases I do not wish to make rash claims, but on the evidence here provided I feel that there is every chance that the new test will prove of real value as a measure of intelligence particularly at higher age levels.

VI. SUMMARY.

1. The new test, to be called the Passalong Test, is presented in the hope that it may be helpful at higher age levels, where good performance tests are still needed.

2. All methods of scoring the test are useful, but apart from valuation of demonstrations, time is the best measure, and is therefore adopted for further work (see Appendix).

3. The application of the test to a group of 13-year-old children shows it to be as good as any single test in the Collins-Drever Scale and better than most of them.

4. The agreement between the results of this investigation and those of other workers which are at all comparable, provides evidence that the position occupied by the Passalong Test in this set of results is likely to be substantiated by further investigation.

5. As the result of our experience with the test we give in the Appendix certain changes which it is proposed to make in the form of the test and in the method of administration and scoring. The purpose of the change is to reduce the time required to give the test, so that it may be used as a unit in a battery of tests.

The author has to thank Dr R. H. Thouless, Head of the Psychology Department at Glasgow University, for suggestions and advice in the course of this investigation. Thanks are also due to the Headmaster of Abercorn Public School, Paisley, whose interest and co-operation made the work of testing in the school possible.

APPENDIX.

Suggested methods of administration and scoring.

In the description of the test which has been given reference was made to the fact that it was not proposed that the test should be used alone but rather as one of a battery of tests as widely divergent in type as possible, so that greater reliability would be secured. For this purpose it is necessary that the test should not take more than say 10 min. to administer, and in its present form it takes a good deal more than that. The purpose of this Appendix is to describe a method which will meet the requirements of time without reducing the value of the test.

Before indicating the changes in administration and scoring which are suggested there is a change in the form of the test which has been shown to be desirable during the present investigation.

As was mentioned in the description of box 7 this box was introduced as a "habit breaker," but as the work proceeded it became clear that it was serving no useful purpose. It is therefore proposed to omit this box from the series altogether. Again in the course of the work there has appeared a need for a new box to form a closer connection between boxes 8 and 9. This is shown by the fact that all the subjects failed in box 9 at the first attempt, whereas that happened with no other box. We therefore suggest the introduction of a new box to form a link between boxes 8 and 9. The new box is shown in Fig. 10. As will be readily seen it is the natural link to connect boxes 8 and 9 and makes the increase in difficulty less extreme.

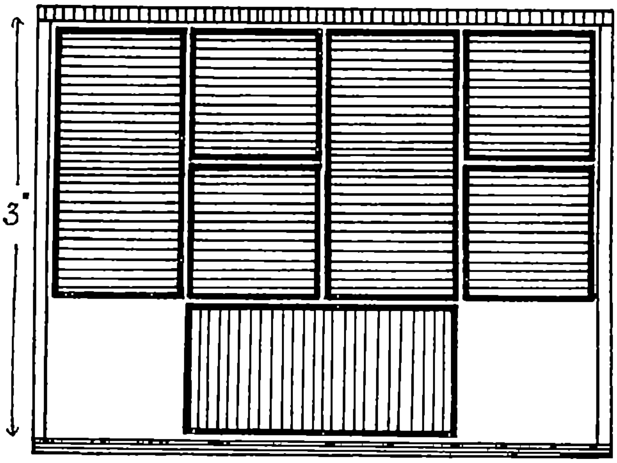

Fig. 10. Showing new box.

Thus the form of the test to be used will be boxes 1 to 6 as described, box 8 to replace box 7 which is to be omitted, the box shown in Fig. 10 to be box 8 and box 9 to remain the final one. We thus have nine boxes of increasing difficulty each leading to the next and depending upon the one preceding.

In the method of scoring already described it was required that every subject should complete every box so that we might be able to assess the value of different methods of scoring; it is now suggested that failure in a box within a definite time limit be used as a criterion of judgment. We suggest that the boxes be presented in rotation with the same instructions as before, but on completion of the first box the subject proceeds to box 2 no matter what time he takes to solve box 1 so long as it is within the limit. His time in each case is noted and that is transferred into a points score in each case (see Table VI). If the subject fails in the time limit given the box is demonstrated and he then proceeds to the *next* box. Failure in two successive boxes means complete failure and the test is stopped. No marks are given where failure is recorded at the end of the time limit and no value is put on demonstrations. Only one trial is given in each box. The method follows closely that given by Collins and Drever for the Painted Cube Test except that they make no deductions such as are here suggested. Table VI shows the method of scoring.

Table VI. *Showing suggested method of scoring.*

Box	Time limit (min.)	Full marks	Deductions
1	2	2	1 for every minute or part thereof more than 1 min.
2	2	3	1 for every 30 sec. or part thereof more than 1 min.
3	3	5	1 for every 30 sec. or part thereof more than 1 min.
4	3	5	1 for every 30 sec. or part thereof more than 1 min.
5	3	5	1 for every 30 sec. or part thereof more than 1 min.
6	3	5	1 for every 30 sec. or part thereof more than 1 min.
7	3	5	1 for every 30 sec. or part thereof more than 1 min.
8	4	7	1 for every 30 sec. or part thereof more than 1½ min.
9	5	8	1 for every 30 sec. or part thereof more than 2 min.
			Possible score 45.

This method[1] has been tried with some 250 subjects and we may indicate very briefly the results so far obtained. The correlation with the National Scale Group Test, form 2 A, remains fairly high. For elementary school cases the figure was 0·60 ± 0·05. As was anticipated the time required to give the test is substantially reduced. The average time is about 15 min. with a variation in certain cases of some 5 min. more or less. This means that the test can be made to form one of a battery for which the total time will not be excessive. On the basis of these 250 cases we give below age norms for this method of scoring.

Table VII. *Showing tentative age norms.*

Age in years	7·5	8·5	9·5	10·5	11·5	12·5	13·5	14·5	15·5
Score	11	13	16	19	21	24	27	29	32

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(5) SPEARMAN, C. (1927). *Abilities of Man.* London, Macmillan and Co.

(6) *Memoirs,* No. 15 (1920). National Academy of Sciences. Washington, D.C., U.S.A.

[1] *Added May 4th,* 1932. Anyone wishing a set of material for the test, with instructions for giving and scoring and tentative norms, should communicate with the author at Hillhead House, The University, Glasgow.

(*Manuscript received* 24 *October,* 1931.)

ON THE COMPUTATION OF REGRESSION EQUATIONS, PARTIAL CORRELATIONS, ETC.

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The following method of calculating the coefficients of regression equations (*i.e.* in our work the weights to be given to scores from a number of tests in order to combine them into a battery or team which will give the highest possible correlation with a given criterion) appears not to be generally known among psychologists, makers of tests, examiners who have to combine marks from several papers, etc., and it has been suggested to me that a short explanatory note might be welcomed in such quarters. The method has for some time been in routine use in our educational laboratory at Moray House, owing to the teaching of Dr A. C. Aitken in Prof. E. T. Whittaker's mathematical laboratory.

Let x_1 be the criterion (*e.g.* the marks of pupils in a School Leaving Certificate) and $x_2, x_3, \ldots, x_n$ be marks in a number of tests or examinations by whose aid it is desired to predict x_1. It is desired to calculate the weights by which to multiply the marks $x_2, x_3, \ldots, x_n$ in order that the weighted sum may correlate as highly as possible with x_1. It is necessary to know all the correlations in the following table:

$$\begin{array}{cccccc}
 & x_1 & x_2 & x_3 & \cdots & x_n \\
x_1 & \cdot & r_{12} & r_{13} & \cdots & r_{1n} \\
x_2 & r_{12} & \cdot & r_{23} & \cdots & r_{2n} \\
x_3 & r_{13} & r_{23} & \cdot & \cdots & r_{3n} \\
\cdots & \cdots & \cdots & \cdots & \cdots & \cdots \\
x_n & r_{1n} & r_{2n} & r_{3n} & \cdots & \cdot
\end{array}$$

It is also necessary to know all the standard deviations $\sigma_1, \sigma_2, \sigma_3, \ldots, \sigma_n$ but in our first consideration of the problem we will suppose that these are equal.

The determinant

$$\Delta = \begin{vmatrix}
1 & r_{12} & r_{13} & \cdots & r_{1n} \\
r_{12} & 1 & r_{23} & \cdots & r_{2n} \\
r_{13} & r_{23} & 1 & \cdots & r_{3n} \\
\cdots & \cdots & \cdots & \cdots & \cdots \\
r_{1n} & r_{2n} & r_{3n} & \cdots & 1
\end{vmatrix} .$$

is formed from the above table of correlations by writing unities in the blank spaces. Then the best weighted team for correlating as highly as possible with x_1 is

$$\frac{\Delta_{12}}{\Delta_{11}} x_2 - \frac{\Delta_{13}}{\Delta_{11}} x_3 + \frac{\Delta_{14}}{\Delta_{11}} x_4 - \dots,$$

the signs continuing to alternate; and since it is only the relative weights which are needed, only Δ_{12}, $-\Delta_{13}$, Δ_{14}, and so on need be calculated, where Δ_{12} is the minor of r_{12}, *i.e.* it is the smaller determinant which results from striking out the first row and second column of Δ, namely,

$$\Delta_{12} = \begin{vmatrix} r_{12} & r_{23} & \dots & r_{2n} \\ r_{13} & 1 & \dots & r_{3n} \\ \dots & \dots & \dots & \dots \\ r_{1n} & r_{3n} & \dots & 1 \end{vmatrix}$$

and similarly for Δ_{13}, Δ_{14} and so on. Non-mathematical readers will find that the numerical example below will make this clear.

The best numerical method for computing the determinants is that devised by Dr Aitken—it combines the best points in Chio's method and Dodgson's ("Lewis Carroll")—and published by him in the *Transactions of the Faculty of Actuaries*, 1931, vol. XIII. It is explained below.

The correlation R of the resulting team with the criterion x_1 will be

$$R = \sqrt{1 - \frac{\Delta}{\Delta_{11}}},$$

where Δ is the whole determinant, Δ_{11} the minor obtained by striking out the first row and first column.

Suppose for example the correlations are as follows (I take single figures for ease in calculating):

	x_1	x_2	x_3	x_4
x_1	.	0·7	0·5	0·2
x_2	0·7	.	0·6	0·4
x_3	0·5	0·6	.	0·5
x_4	0·2	0·4	0·5	.

Then the complete determinant is

$$\Delta = \begin{vmatrix} 1 & 0{\cdot}7 & 0{\cdot}5 & 0{\cdot}2 \\ 0{\cdot}7 & 1 & 0{\cdot}6 & 0{\cdot}4 \\ 0{\cdot}5 & 0{\cdot}6 & 1 & 0{\cdot}5 \\ 0{\cdot}2 & 0{\cdot}4 & 0{\cdot}5 & 1 \end{vmatrix}$$

I shall explain Aitken's method by computing this complete determinant.

Choose one of the elements of the determinant to act as the 'pivot.' In our work it is best to choose the second unity in the diagonal, italicized above, (*a*) because, being unity, it facilitates a later division, (*b*) because it keeps the work axisymmetrical, forming a check, and (*c*) because we can then simultaneously calculate Δ_{11}.

Form a new and smaller determinant by calculating every 'tetrad difference' of which the pivot is a member and setting them down in order. It is an advantage of the method that tetrad differences are usually needed in any case for other reasons, so that computation is economized.

This new determinant, divided by the pivot, is of the same value as the old, *i.e.*

$$\Delta = \begin{vmatrix} 0{\cdot}51 & -0{\cdot}08 & 0{\cdot}08 \\ -0{\cdot}08 & 0{\cdot}\mathit{64} & 0{\cdot}26 \\ 0{\cdot}08 & 0{\cdot}26 & 0{\cdot}84 \end{vmatrix} \div \text{Pivot (here 1).}$$

For example $0{\cdot}51$ is $(1 \times 1 - 0{\cdot}7 \times 0{\cdot}7)$,

$0{\cdot}26$ is $(1 \times 0{\cdot}5 - 0{\cdot}4 \times 0{\cdot}6)$.

Now choose a new pivot in this determinant, either one which it is easy to divide by later, or again the second element in the diagonal. As a rule there will not be (since several decimal places will be used in practice) any easy divisor, so choose the second element in the diagonal (unless it is very small or otherwise peculiar).

Repeat the process of calculating those tetrad differences of this smaller determinant which involve the new pivot, but before writing them down, divide them by the *preceding* pivot. (In our work this will not be necessary here, since we chose a unity for the first pivot, but the rule should be remembered, and it will be necessary at all later reductions.) We obtain

$$\Delta = \begin{vmatrix} 0{\cdot}3200 & -0{\cdot}0720 \\ -0{\cdot}0720 & 0{\cdot}4700 \end{vmatrix} \div \text{Pivot } (0{\cdot}64).$$

For example, $0{\cdot}4700$ is $(0{\cdot}64 \times 0{\cdot}84 - 0{\cdot}26 \times 0{\cdot}26)$.

The process is repeated, if the original determinant is large, until, as here, we are left with a single tetrad. Finally

$$\Delta = \frac{0{\cdot}3200 \times 0{\cdot}4700 - (-0{\cdot}0720)^2}{0{\cdot}64} = \frac{0{\cdot}145216}{0{\cdot}64}$$

$$= 0{\cdot}2269.$$

If all the decimals have been retained it is a check that each division by a pivot should, as here, leave no remainder; but in the practical cases, where the original correlations contain at least two decimals, it will not be usual to retain all the decimals unless a calculating machine is being employed. Logarithms are not recommended. A Crelle's multiplication table is very useful.

If the reader now looks back over the work he will see that Δ_{11} has simultaneously been calculated, and appears in the last stage as 0·4700. We have therefore, for the highest correlation which can possibly be obtained from the team x_2, x_3 and x_4 with x_1 the value

$$R = \sqrt{1 - \frac{\Delta}{\Delta_{11}}} = \sqrt{1 - \frac{0{\cdot}2269}{0{\cdot}4700}} = 0{\cdot}719,$$

which is not a very great improvement on the largest single correlation (0·700) with which we started; but sometimes the improvement is marked.

The weights which will give this correlation 0·719 are Δ_{12}, $-\Delta_{13}$ and Δ_{14}, and it will be found that parts of these have already been calculated.

$$\Delta_{12} = \begin{vmatrix} 0{\cdot}7 & 0{\cdot}6 & 0{\cdot}4 \\ 0{\cdot}5 & \mathit{1} & 0{\cdot}5 \\ 0{\cdot}2 & 0{\cdot}5 & 1 \end{vmatrix} = \begin{vmatrix} 0{\cdot}40 & -0{\cdot}10 \\ & \\ 0{\cdot}05 & 0{\cdot}75 \end{vmatrix} = 0{\cdot}305$$

$$\Delta_{13} = \begin{vmatrix} 0{\cdot}7 & \mathit{1} & 0{\cdot}4 \\ 0{\cdot}5 & 0{\cdot}6 & 0{\cdot}5 \\ 0{\cdot}2 & 0{\cdot}4 & 1 \end{vmatrix} = \begin{vmatrix} -0{\cdot}08 & 0{\cdot}26 \\ & \\ 0{\cdot}08 & 0{\cdot}84 \end{vmatrix} = -0{\cdot}088$$

$$\Delta_{14} = \begin{vmatrix} 0{\cdot}7 & 1 & 0{\cdot}6 \\ 0{\cdot}5 & 0{\cdot}6 & \mathit{1} \\ 0{\cdot}2 & 0{\cdot}4 & 0{\cdot}5 \end{vmatrix} = \begin{vmatrix} 0{\cdot}40 & 0{\cdot}64 \\ & \\ 0{\cdot}05 & -0{\cdot}10 \end{vmatrix} = -0{\cdot}072$$

(In each case one of the unities is chosen as pivot and has been italicised.)

The prediction equation for x_1 is therefore

$$305x_2 + 88x_3 - 72x_4,$$

or, dividing through by one of the weights

$$4{\cdot}24x_2 + 1{\cdot}22x_3 - x_4.$$

Notice that the marks of test x_4 are here subtracted—this arises because x_4 correlates so much more with its fellow members of the team than it does with the criterion x_1.

We have up to this point supposed that all the standard deviations are equal. As a rule this will not be the case, and the weights applicable to the actual raw scores will be

$$\frac{\Delta_{12}}{\sigma_2}, \ -\frac{\Delta_{13}}{\sigma_3}, \ \frac{\Delta_{14}}{\sigma_4}.$$

Several other formulae are much simplified by the determinantal notation and Aitken's method of computation. For example, the partial correlation

$$r_{12\cdot 3456} = \frac{\Delta_{12}}{\sqrt{\Delta_{11}\Delta_{22}}}.$$

SUMMARY.

This note draws the attention of psychologists and others to the advantages of the determinantal method of calculating regression coefficients, partial correlations, etc., when Dr A. C. Aitken's method of computation is used (*Transactions of the Faculty of Actuaries*, 1931, vol. XIII).

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A GESTALT THEORY OF HUMOUR.

By NORMAN R. F. MAIER.

(*From the University of Michigan.*)

I. *Preliminary observations* (p. 69).
II. *Suddenness* (pp. 69–71).
III. *Objectivity* (pp. 71–72).
IV. *The ridiculous* (pp. 72–73).
V. *Summary* (pp. 73–74).

I. Preliminary observations.

The literature on the subject of the humorous is profuse, but no satisfactory psychological theory is yet available. The existent literature has largely confined itself to pointing out certain characteristics in various humorous stories: incongruity is perhaps the most generally accepted characteristic, having been first pointed out by Aristotle. Our present purpose is to study the mental processes involved in the humorous experience. No matter what the objective criteria of a joke are, it is not humorous unless a certain characteristic mental process is aroused by it. Classifying stories that arouse a humorous experience is therefore somewhat artificial, and theories based on the selection of common elements are likely to be inconsistent and unsatisfactory.

II. Suddenness.

The findings of Gestalt psychology combined with the results of certain experiments on reasoning, seem to explain the relevant mental processes of the humorous experience.

Wertheimer (1) has shown that the meaning of elements depends on the configuration of which they are a part. When the configuration suddenly changes, the meaning of the elements suddenly changes as a consequence. The writer has shown (2, 3) that direction is a determining factor underlying the formation of configuration. A problem is always looked at from a certain point of view and this point of view determines what one will do about it (*i.e.* what direction one's mind will take). A particular direction facilitates certain configurations and inhibits others. Thus when we are presented with any facts we tend to organize them in a certain way. Usually past experience gives us the point of view; we organize the facts accordingly and consequently miss a new organization or interpretation. A humorous incident is told so as to encourage a certain point of view. Then in the end we are given a conclusion (an organization of the facts presented) which is very different from the one we anticipated. It is like the experience of insight except for certain differences which we shall point out later.

When the final configuration is experienced in a humorous situation, it is due to direct stimulation. That is, we are told the conclusion; we do not have to find it. In reasoning the final configuration is found by the reasoner, and is therefore in harmony with his direction. If the solution to a problem is not found, but is shown to the person attempting the problem, then the experiencing of the configuration is that of understanding. If the answer to the problem is not in harmony with the direction in his thinking, the solution comes as a surprise, and laughter often results. Suddenly finding the solution to a problem or having an unexpected answer shown to one is always pleasant if the feeling about the consequences of the solution are disregarded. Thus far we have discovered that the humorous experience and reasoning or understanding are not unlike.

Not only must a configuration be formed in a humorous experience, but in every case it must appear suddenly, the listener or reader having been unprepared for it. Because the reader is unprepared for the final configuration, it appears very suddenly and unexpectedly: the moment he sees the point of a story he is experiencing the final configuration and its resultant meanings. Since he has been prepared for something else, the unexpected configuration is a surprise: for a moment there is confusion of thought, then suddenly the entire situation is clarified.

The importance of suddenness in the humorous experience has been pointed out by nearly every writer on the comic. For example, Kant(4) accounted for it by his theory of the sudden release of strains of expectation. Bergson(5) implies it when he stresses the machine-like nature of comic acts. Gilman(6) points out the necessity of the surprise element, but does not explain the nature of surprise.

Theories of Incongruity also seem to imply the importance of surprise or suddenness. Incongruity generally implies lack of harmony between what we expect (on the basis of past experience) and what we see or what actually happens. This conflict between the concept and the percept appears suddenly. The story suddenly leads to an ending which is not in harmony with what was expected. There is thus incongruity between the beginning and the end of the story.

If we say that the experiencing of an unexpected configuration is not in harmony with what we expected, and that configurations appear suddenly, then the present theory accounts for everything which theories of incongruity explain, without having the same limitations.

But the unexpected and sudden appearance of configurations is not sufficient to characterize the humorous experience. Understanding may involve these characteristics, and suddenness may arouse fear as well as a humorous experience. We must therefore seek further differences between the humorous and the other experiences.

III. OBJECTIVITY.

Bergson ((5) p. 139) points out that the content of humorous subject-matter cannot be subjective; it must be objective. This seems to be a very important observation, although he leaves it relatively undeveloped. Plato, however, long before stated that wit and humour must be without pain. Later writers have also shown how certain emotions limit the field of the comic, but they have not insisted on pure objectivity.

It is possible to experience humour only when we do not sympathize, or feel, or implicitly participate with the thing we regard as humorous. To see an old lady slip and fall on an icy pavement causes some to laugh, while others feel only sorrow. Those who laugh see the situation as objective, and objectively there is no tragedy in the situation. Those who fear for the old lady's safety see the situation subjectively; their emotions are aroused and for them it is humourless. We can laugh only so long as our feelings and sympathies are kept out of the situation.

In a moving picture comedy the hero is thrown out of a window and we laugh; in a tragedy the hero receives the same treatment and we are terror stricken. What makes the difference? The difference lies entirely in the setting in which such behaviour is imbedded: and because of such differences in setting, our attitude in the one case is entirely objective, in the other case subjective. In the tragedy our emotions and sympathies are played upon and we feel for the hero; in the comedy we find no subjective content: we are aloof and merely look on.

If we ask people to tell humorous stories about their past adventures, we often hear tales of incidents which at the time of their happening were tragic. Events of our childhood, and even last year's tragedies, are suddenly funny to us as we tell them to evoke laughter in others. Time has removed the subjective factors and we are therefore able to see the situation objectively.

Objectivity, however, may also characterize productive thinking. In fact, productive thinking differs from creative imagination in that the elements of its configurations are largely objective. We must therefore seek further for a distinction between the humorous and the reasoning experiences.

IV. THE RIDICULOUS.

The distinction is found in the element of the ridiculous which is present in the humorous experience, but which is not present in reasoning. This factor has been variously pointed out by different writers. For example, Kant(7) makes the strains of expectation become transformed into nothing. Gilman(8) points out that the comic often contains a comparison between the possible and the impossible or probable. Freud(9) refers to the pleasure in nonsense, and Sully(10) to the play attitude in the comic.

These slight differences we wish to harmonize under the ridiculous, and show that the ridiculous plays a part in all humorous experiences, and not only in certain select cases. First, however, we must examine the nature of the ridiculous.

The ridiculous is logical only within the bounds of certain facts. It appears logical in a certain setting, but as soon as we get out of the setting and take other facts into consideration, the logic is lost. Humour, therefore, may be inconsistent with reality as a whole. Tragedy is not: it has no element of the ridiculous. Because the ridiculous has only a limited logic, it is easy for us to take it lightly. It therefore encourages an objective attitude. We do not take a ridiculous situation seriously because it is not meant to be part of reality. A ridiculous explanation of a problem in a humorous story is thus quite out of the realm of our experience, and it is therefore less likely to be anticipated. The final configuration, the ridiculous conclusion of a story, can thus be experienced with great suddenness.

When a story is being told, we are willing to accept any assumptions so long as they are consistent one with another. Thus we accept the fact that the man in a funny paper is strong enough to lift a house, or to hit another man on the chin and knock him over the roof; but a person with logical sense cannot also accept the fact that this same strong man can be jerked out of a boat by a small fish at the end of his line. Such an idea does not harmonize with the other facts of the story, and therefore disgusts the intelligent reader. If anyone treats situations of this kind as comic it is because he has not taken them as a whole, but enjoys each scene separately.

The humorous situation is thus only 'momentarily true' for the facts presented: it need not be 'true' pragmatically. The humorous situation is then a very isolated thing, and its configurations are not to be taken seriously. They are not to be refuted, and for one to try and refute them indicates that one is without humour. Thus when we say that an individual has no sense of humour, we mean that he cannot accept a situation as being only 'momentarily true.' That is, he insists upon applying the pragmatic test to *every* situation. Productive thinking, although it may deal with an isolated situation, must be in harmony with other experiences and data outside the bounds of that situation. Its configurations must be subjected to generalization: they must be true. Science deals with universals; these are profound and affect our lives. Although the elements making up configurations in science are often entirely objective, the configurations themselves arouse the subjective reaction. The configurations become evaluated and evaluation always involves a subjective reaction. In that the contents of the ridiculous are isolated from reality and its assumptions willingly accepted, it resembles a play attitude. The ridiculous element of the humorous experience therefore accounts for what Sully regards as the play element in the comic.

The importance of the ridiculous was definitely stressed by Kant when he wrote: "Humour in the good sense means the talent of being able to put oneself voluntarily into a certain mental disposition in which everything is judged quite differently from the ordinary method (reversed, in fact), and yet is in accordance with certain rational principles in such a frame of mind"(11). He also stated that "Jest must contain something that is capable of deceiving for the moment"(12).

Thus in the limited and definite sense in which the ridiculous has been explained above, humour differs from reasoning or understanding only in so far as it contains the elements of the ridiculous.

V. SUMMARY.

The thought-configuration which makes for a humorous experience must (1) be unprepared for; (2) appear suddenly and bring with it a change in the meaning of its elements; (3) be made up of elements which are experienced entirely objectively (no emotional factors can be part of the configuration); (4) contain as its elements the facts appearing in the story, and these facts must be harmonized, explained and unified; and (5) have the characteristics of the ridiculous in that its harmony and logic apply only to its own elements.

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(11) *Op. cit.* p. 228.

(12) *Ibid.* p. 225.

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ERRATUM.

In "Further Notes on the Single General Factor in Mental Measurements," by J. C. Maxwell Garnett, in this *Journal* (1932), xxii, formula (1) on p. 364 should read:

$$\frac{r_{ac}}{r_{ad}} = \frac{r_{bc}}{r_{bd}} \text{ instead of } \frac{r_{ac}}{r_{ad}} = \frac{r_{bc}}{r_{cd}}.$$

PUBLICATIONS RECENTLY RECEIVED.

The Social Life of Monkeys and Apes. By S. ZUCKERMAN. London: Kegan Paul, Trench, Trubner & Co., Ltd. 1932. Pp. x + 357. 15*s*. net.

This interesting study of monkey society is based principally upon detailed observations of the behaviour of colonies of the Hamadryas baboon, and especially of the colony living on "Monkey Hill" in the London Zoological Gardens. The author considers that the social life of monkeys resembles that of humans, and is differentiated from that of the lower mammals, in that it is conditioned by an uninterrupted reproductive life; there is no reliable evidence of any demarcated breeding season, although the sexual attractiveness of the female varies with the phases of the menstrual cycle. The sexual behaviour of the baboon is extremely diffuse and varied, beginning long before puberty. Its exact nature is frequently determined by the interaction, in a society based upon dominance, of the dominant qualities of any animal with those of its fellows. Owing to lack of intelligence, the social behaviour of monkeys is determined reflexly, rather than by any ideational thinking, a characteristic which differentiates it from human behaviour. Apart from this qualification, there is an almost startling resemblance between the monkey and the human societies. These most interesting conclusions are admirably brought out by the author, though some of his deductions should perhaps be qualified by the fact of the great and unnatural predominance of males over females in the societies which he has studied. This otherwise excellent book would, however, be improved by the omission of certain repetitions, and of its unnecessarily detailed treatment of the physiology of reproduction.

Experimental Psychology. By JOHANNES LINDWORSKY. Trans. by Harry R. De Silva. London: George Allen & Unwin. 1931. Pp. xix + 406.

Prof. Lindworsky's excellent general survey of psychology was first published in 1923 and is already well known in this country. It is indeed rather a stretching of terms to speak of the book as "experimental psychology." For the author is obviously most keenly interested in those aspects of psychological study which are least of all amenable to exact experimental treatment. But, as everybody now knows, he is throughout clear, judicious, unbiased and anxious above all to keep closely in touch with fact. On the side of the study of the special senses most students will find his book hardly adequate. All readers however will find very much to learn from his treatment of the "higher mental achievements," and the socially influenced mental processes. It is, in fact, for its full discussion of these topics that the book stands out from most of the other text-books in the subject.

Dr De Silva has accomplished a difficult task of translation exceedingly well.

A History of Psychology in Autobiography. Vol. II. Edited by C. MURCHISON. Clark University Press. (London: Humphrey Milford.) 1932. Pp. xv + 407. 28*s*. net.

No doubt every psychological library already possesses a standing subscription to this very important series of volumes. But if there are any that do not they should hasten to obtain one. Teachers will get much entertainment and help and students a great amount of stimulation from their pages. This volume contains autobiographical notes by: Benjamin Bourdon, James Drever, Knight Dunlap, Guilio Cesare Ferrari, Shepherd Ivory Franz, Karl Groos, Gerardus Heymans, Harald Höffding, Charles H. Judd, C. Lloyd Morgan, Walker B. Pillsbury, Lewis M. Terman, Margaret Floy Washburn, Robert S. Woodworth and Robert Mearns Yerkes. They are all intensely interesting in very different ways, and perhaps they might be made the material of a useful experiment upon the reliability of an autobiography as an indicator of personality and temperament. Perhaps it is inevitable that they should leave a general impression that psychologists in general "think no small beer" of themselves. But at the same time they give a very pleasing indication of readiness, even anxiety, to give credit where credit is due, and of a refreshing willingness to be as frank as possible. It is interesting that most of the authors—and there are many—who trace their early inspiration to Wundt—especially disclaim the very things for which Wundtian teaching has recently been most violently attacked. Another interesting feature is the almost universal disapproval of the more wild aspects of behaviourism. In one respect it seems probable that future histories in autobiography will differ very markedly from this. With hardly an exception, all the psychologists represented here found their way into psychology through some study or other of related branches of philosophy. Perhaps that has something to do with the mass of 'systems' that are current nowadays. It will be a matter of profound interest to see what will happen as the leaders in psychology come more and more from men and women trained in the methods and ideals of positive science. This book, and its fellow volumes, should be available for every student of psychology.

Fields of Psychology. A Study of Man and his Environment. By G. D. HIGGINSON. London: Grant Allen & Unwin, Ltd. 1931. Pp. xviii + 613. 15*s*. net.

This is a very comprehensive general text-book of psychology addressed to the student who has already some knowledge of the subject. It deals first with systematic studies: structural and functional psychology, behaviourism and the *Gestalt* school. Next it takes up problems of development discussing in general the significance of evolution for psychology, the characteristics of animal behaviour, the growth of the human race, the development of the individual, and the psychology of the social group. A third part takes up problems of applied psychology in education, industry, the economics of distribution and law. Finally it considers problems of abnormal psychology. The general point of view throughout is functional and the author is admirably on his guard against undue systematisation. Rather too much ground is covered for anything to be done very thoroughly, but the writing is clear and interesting, and some very good bibliographies are given.

L'Année Psychologique. Publié par HENRI PIÉRON. Paris: Félix Alcan. 1930. Tome I and II. Pp. 1111. 120 fr.

The original articles in this volume are as follows: "Do blind people possess a sense of obstacles?" by W. Dolanski; "The visual threshold of duration," by G. Durup and A. Fessard; "Auditory theory," by G. v. Békésy; "A contribution to the study of gifted children," by O. Decroly and M.-L. Wauthier; "The development of visual representation in relation to the initial impression," by M. Rostohar; "Notes on the technopsychology of industrial work," by L. Walther; "Contributions to the study of the course of learning," by D. Heller-Kowarski, L. Kowarski and M. François. Shorter notes appear as follows: "The notion of validity," by A. Fessard and H. Piéron; "Subjective factors in examiners' judgments," by A. Laugier and D. Weinberg; "A measure of motor suggestibility," by J.-M. Lahy; "The minimum measures necessary for gauging a test," by A. Fessard and H. Piéron. There are also brief abstracts of 1690 articles, and the usual chronicle of events. This number fully maintains the very high reputation won by the earlier issues.

Meaning and Change of Meaning, with Special Reference to the English Language. By GUSTAF STEIN. Göteborg: Wettergren and Kerbers. Göteborgs Högskolas Årsskrift, XXXVIII, 1931.

It is impossible within the limits of a brief notice to do justice to this exceedingly able and valuable monograph. Every psychologist who is interested in those problems of speech which have to do with its functions in the expression and transmission of meaning should read it with care. He will find a large number of original and striking illustrations, an admirable account of many different theories, and a balanced judgment concerning numerous disputed points. He will find also a great many matters which, if he has the will and the skill, he might set to work to investigate by experimental methods. The book is a notable contribution to its thorny topic and should become widely known. There is an excellent bibliography, though it is not, and perhaps could hardly reasonably claim to be, complete.

Urteil und Beurteilung. Von Walter Blumenfeld. Dritter Ergänzungsband, Archiv f. d. Gesamte Psychol. Leipzig. 1931. 2 vols.. Part I, R.M. 26; part II, R.M. 22.

The first volume is mainly concerned with an exhaustive analysis of philosophical and psychological theories of judgments and the judging process. The final conclusion is the somewhat negative one that all these theories are inadequate because all are necessarily one-sided. The epistemological approach suffocates in its armoury of definitions as soon as it is put to the test of the real world of objects; the phenomenological approach fails on the epistemological side. The only hopeful way out of the tangle is exhaustive experimentation on the processes that precede the act of judgment. This way is taken in the second volume. Experiments are described on the comparison of series and groups of points, the perception of movement, and the classification of figures. It is shown that the processes of judging depend on the structure and order of the qualitative series, the position of the differential steps in the series, and on the attitudes of the person making the comparisons. These results are of value in again forcing us to realize the importance of attitudes in all judgments as to the order of merit of children and their work, or in the ordering of psychotechnic test results.

The experimental results have a bearing on the Weber-Fechner law, which is again confirmed in its broad principles. In discussing the theoretical implications of his results the author thinks he has found a great deal of correspondence between weakly damped vibrations and the description of judgments in terms of the typical experiences of doubt that precede them. There is no analogy to chemical processes, he says, and we must therefore agree with Köhler as to the electrical properties and processes in the whole nervous sector from the peripheral organ to the central organ. Here the author, whose criticisms are usually clear and cogent, would seem to have fallen into the same error as the 'Gestaltists.' He has substituted causal for ontological analogies.

The Scientific Basis of Social Work: A Study in Family Case Work. By Maurice J. Karpf. New York: Columbia University Press. (London: Humphrey Milford.) 1931. Pp. xix + 424. 24*s.* 6*d.* net.

The author discusses the following questions: What knowledge do social workers need? What knowledge do social workers use? What knowledge do social workers receive? How may social work acquire a scientific basis? The whole forms a valuable survey of the present aims of Social Work in America, and of the type of training which the social worker is offered. While the author is convinced of the extreme importance of organized efforts to secure social amelioration, he is very well alive to the inadequacies of present methods and the training upon which they are based. His actual suggestions are, as is perhaps inevitable, somewhat less definite than his criticism, but as a balanced, reasonable and constructive survey the book is to be welcomed.

Constitution-Types in Delinquency. By W. A. Willemse. London: Kegan Paul, Trench, Trubner & Co., Ltd. International Library of Psychology, etc. 1932. Pp. viii + 266. 15*s.*

The considerable growth of "type" psychologies has been a leading characteristic of recent psychological and clinical study and speculation. This book is based mainly on the "constitution-typology" of Kretschmer, but makes use also of the different classifications of Jaensch, Pende, Berman, Heymans, Wiersma and others. The various doctrines discussed are very clearly described and the claims made for them considered. In general the author's attitude is favourable, but his many incidental criticisms are often well directed. The original part of the book consists of a large number of first-hand case studies. Although these are full of interest one gets an impression, as in much of this type of work, of a certain lack of thoroughness in clinical examination, a certain willingness to accept somewhat obvious groupings as of fundamental importance. Still the book collects a great amount of material, and contains numerous suggestions that should be followed up by further study. It is undoubtedly an important contribution to this rapidly growing branch of psychological study.

The Defeat of Baudelaire. By RENÉ LAFORGUE. Trans. by Herbert Agar. London. 1932. The Hogarth Press. International Psycho-Analytical Library, No. 21. Pp. xiv + 192. 10*s.* 6*d.*

This excellently translated book is "a psycho-analytical study of the neurosis of Baudelaire." Baudelaire provides plenty of easy material for the psychoanalyst and a great amount of this is made use of with much skill in the present study. The primary root of Baudelaire's troubles is said to be a profound Oedipus complex. Numerous illustrations are given both from Baudelaire's life and from his writings. The study is particularly well written. There is an interesting, if somewhat one-sided, chapter on the psychology of artistic creation.

What Life should mean to You. By ALFRED ADLER. London: George Allen & Unwin, Ltd. 1932. Pp. 300. 10*s.* 6*d.* net.

Dr Adler must find the writing of books exceedingly easy. At any rate he produces a large number of them, and they all bear a close family likeness. This one contains much good advice and many stories of his successful dealings with his patients. In spite of his long catalogue of life's troubles they are all made to appear rather simple; "Individual Psychology has found no problems in life which cannot be grouped under these three main problems—occupational, social and sexual." Hence, we are left to believe, there are no others. These three classes of problems are further simplified, for they all arise from the fact that individuals, being clearly or obscurely aware of a weakness in some particular direction, react in the opposite way and in an exaggerated manner. Most of the abundant moralising is quite good and may have sounded very convincing on the public platform. In a book much of it appears thin and drawn out, and in practice it will be less simple than it seems.

Speech Pathology. By LEE EDWARD TRAVIS. London: D. Appleton & Co. 1931. Pp. xxxiv + 331. 18*s.* 6*d.* net.

The best part of this book is its practical part. After a somewhat scrappy and not over clear discussion of the neuromuscular basis of speech and of the causes of speech disorders, Dr Travis gives an excellent detailed description of the technique of speech examinations, and embarks upon a deeply interesting discussion of stuttering. He is convinced that stuttering is fundamentally due to some loss of central function, but here, also, the theory is extremely speculative and the exact mechanism involved is never made very clear. The book owes its real justification to the facts which he has collected concerning stuttering and to the author's accounts of his methods of treatment. These deserve to be widely read, and will be of great value to all who are in a practical sense concerned in the study of defects of speech.

Abnormal Psychology: its concepts and theories. By H. L. HOLLINGWORTH. London: Methuen & Co. 1931. Pp. xii + 556. 15*s.* net.

A sound general introduction to abnormal psychology, free from the exaggerations of any particular controversial theory, was very greatly needed. Prof. Hollingworth has, in this excellent book, almost, if not quite, supplied the need. The volume is consistently readable, well balanced and very well informed. Clear accounts are given of practically all the important methods and theories in the wide field that is covered, and perhaps also of some unimportant ones. Wherever critical remarks are called for they are given in a careful and lucid manner. There is a bibliography which on the whole is admirably selected.

If anything the volume is a little on the long side. This is due less to the comprehensive survey attempted than to a certain liking for a somewhat leisurely style. The book could be improved by a careful pruning. But as it is, it is much better than most of the existing texts, and it should speedily take its place as a very widely read account suitable both for the educated general reader and for the student.

A Text-book of Psychiatry for Students and Practitioners. By D. K. HENDERSON and R. D. GILLESPIE. Oxford Medical Publications. 1932. Pp. ix + 595. 1932. (Third edition.) 18*s.* net.

It is most encouraging to find a third edition of this very excellent text-book already demanded. The authors have added a chapter on the Psychiatry of Childhood which forms a sane and good introduction to this subject. They have also dealt briefly but clearly with some of the legal questions involved in psychiatry in the light of recent enactments. For the rest the whole book has undergone a careful revision, and in particular, a good deal has been added to the discussion of Epilepsy. There is certainly no better book than this as a general introduction to psychiatry. It maintains a consistently high standard throughout.

Clinical Psychology. Studies in Honour of Lightmer Witmer to commemorate the thirty-fifth anniversary of the founding of the First Psychological Clinic. Edited by ROBERT A. BROTEMARKLE. Oxford University Press. 1931. Pp. xxi + 409. 20*s.* net.

This interesting volume contains the following studies: "Lightmer Witmer: a biographical sketch," by Joseph Collins; "History of the Psychological Clinic," by S. W. Fernberger; "Mental Deficiency," by F. N. Maxfield; "The Superior Child," by Alice M. J. Rockwell; "The Pre-School Child," by Dorothy K. Hallowell; "School Levels," by Miles S. Murphy; "The Correction of Speech Defects," by E. B. Twitmyer; "Special Education," by Gladys G. Ides; "College Student Personnel Work," by R. A. Brotemarkle; "Industry," by M. S. Viteles; "Individual Mental Testing," by H. J. Humpstone; "Group Mental Tests," by Reuel H. Sylvester; "Psychobiochemistry," by H. E. Starr; "Diagnostic Education," by A. Phillips; "Private Practice," by David Mitchell; "General Psychology," by S. Smith; "The Principles of Education," by Anna J. McKeay; "The School Counsellor," by Anna E. Biddle; "College Admission," by Karl G. Miller; "Teacher Training," by J. D. Heilman; "Teacher Training for Special Education," by F. H. Reiter; "The Social Sciences," by F. Paschal; "Social Service Work," by Charlotte E. Grave; "Moral Training," by Earl S. Rudisill; "Conduct Problems," by Clara H. Town; "Criminology," by R. H. Gault; "Religious Experience," by Dallas E. Bazby. Then also three reprints of papers by Dr Lightmer Witmer: "Clinical Psychology"; "On the Relation of Intelligence and Efficiency" and "Psychological Diagnosis and the Psychonomic Orientation of Analytical Science."

Anger in Young Children. By FLORENCE L. GOODENOUGH. University of Minnesota Press. 1931. Pp. xiii + 278. $2.50.

This is a very valuable monograph consisting of an observational study of fairly wide range. The incidence of anger outbursts in male and female children, their frequency, duration, conditions and after effects are all investigated and the results of the investigation set forth clearly with a minimum of speculation. Methods of control generally adopted and their apparent efficacy are also reported. There is a sensible chapter of the "parent-child relationship." A bibliography is appended.

The Children we Teach: Seven to Eleven Years. By SUSAN ISAACS. London: University of London Press. 1932. Pp. 176. 3*s.* 6*d.* net.

Dr Isaacs has here produced an exceedingly good short book, excellent alike for parents and teachers. She is concerned directly with the outstanding characteristics of children of a relatively early age in relation to their education. She deals first with Individual Differences, then with Social Development and finally with Intellectual Development. All her points are expressed with remarkable lucidity and without forbidding technicality. She is always ready with first-hand illustrations and she preserves a wholly admirable openmindedness throughout. The last section on Intellectual Development is particularly good and timely and should go far to correct current opinions, which seem to be based largely on the adult desire to demonstrate his superiority. The book can be recommended without reserve.

Education of the Backward Child. By D. KENNEDY FRASER. London: University of London Press. 1932. Pp. 254. 6*s.* net.

The bent of this book is practical. The author discusses backwardness and how it may be diagnosed, deals with its relations to organization in school and class, and tackles the special problems of teaching language, oral and written, handwork, reading, composition and number. He deals briefly also with older children, and with the mongol, and concludes by a consideration of the teacher's attitude. Careful illustration is given whenever recommendations are made.

Experiments in Educational Self Government. By A. L. GORDON MACKAY. London: George Allen & Unwin, Ltd. 1931. Pp. 272. 7*s.* 6*d.* net.

The author wrote the book as a result of a direct experimental attempt by himself to conduct classes, both in School and at the University, so as to allow the greatest possible scope for individual and group self government. The account is deeply interesting and raises a large number of attractive psychological problems. Every teacher would probably be the better for reading it, and psychologists will find in it much worthy material for their study. Sufficient detail is given to allow anybody who cares to attempt a repetition of the experiment.

A Handbook of Child Psychology. Edited by CARL MURCHISON. Clark University Press. International University Series. 1931. (London: Humphrey Milford.) Pp. xii + 711. 22*s.* 6*d.* net.

Once again Prof. Carl Murchison has performed a good service to psychology. He has collected and edited a series of interesting articles which cover a large field of child psychology. It is inevitable that the essays should be of varying merit, but most of them are the direct outcome of original and first-hand observation and the whole forms a notable contribution to its rapidly developing subject. The topics and authors are as follows: "The Methods of Child Psychology," by J. E. Anderson; "Eating, Sleeping and Elimination," by Helen T. Woolley; "The Conditioning of Children's Emotions," by Mary Cover Jones; "Environmental Forces in Child Behaviour and Development," by Kurt Lewin; "The Experimental Construction of an Environment Optional for Mental Growth," by Susan Isaacs; "The Developmental Psychology of Twins," by A. Gesell; "Order of Birth in Relation to the Development of the Child," by H. E. Jones; "Physical Growth and Motor Development and their Relation to Mental Development in Children," by Beth L. Wellman; "Language Development," by Dorothea McCarthy; "Learning in Children," by J. Peterson; "Children's Philosophies," by J. Piaget; "The Social Behaviour of the Child," by Charlotte Bühler; "Children's Morals," by V. Jones; "Children's Drawings," by Florence L. Goodenough; "Children's Plays, Games and Amusements," by Helen Marshall; "Children's Dreams," by C. W. Kimmins; "Psychoanalysis of the Child," by Anna Freud; "The Gifted Child," by L. M. Terman; "Feeblemindedness," by R. Pintner; "Special Gifts and Special Deficiencies," by Leta S. Hollingworth; "The Eidetic Child," by H. Klüver; "The Primitive Child," by Margaret Mead.

Diary of a Child's Life. By VILHELM RASMUSSEN. Trans. by Margery Blanchard. London: Simpkin Marshall, Ltd. 1931. Pp. 187. 6*s*. net.

This is an account, slightly written up from notes made at the time of observation, of the developing life of a girl from the first to the fourteenth year. It is interesting in itself, though the observations are not particularly penetrating, and a good many of them are very slight. It is greatly to be hoped that the author's desire that his diary should stimulate many others to keep similar records may be realized. The most valuable part of this record is the early part.

Le Choc de Patriotismes: Les Sentiments Collectifs et la Morale entre Nations. Par EDMOND PRIVAT. Paris: Félix Alcan. 1931. Pp. vii + 179. 15 fr.

This little book is exceedingly well worth reading. It is written with a sort of breathless enthusiasm and spirit, and with very remarkable decisiveness. M. Privat rapidly traces the growth of national patriotism and illustrates its consequences in the modern world in many different directions. His thesis is that two apparently opposed movements have grown up in society. The one develops a narrow but vigorous group sentiment, and the other a wider kind of collectivism. He thinks that both are likely to continue, but that probably they will tend in the future to come more closely together. While there is nothing very new in his argument, his way of stating it is striking, and he makes numerous statements which will provide the social psychologist with much food for thought.

Youth and Sex. By MEYRICK BOOTH. London: Grant Allen & Unwin, Ltd. 1932. Pp. 224. 5*s*. net.

This book is put forward as "a psychological study." It is in fact a rather popularly written discussion, interesting enough to read, from which nothing very definite emerges. The main upshot appears to be that the modern world is in very rapid change, that current social and moral values are exceedingly indefinite, that there is a tendency to emphasize far too strongly an ideal of complete emancipation from all forms of social control, and that these put together produce some difficult problems.

The Mind in Conflict. By R. A. HOWDEN. Oxford University Press. 1931. Pp. xiii + 82. 2*s*. 6*d*. net.

This book is described as "A simple explanation of how modern Psychology sets out to solve Nervous difficulties and Problems." On the whole it is clear and unbiassed and about as "simple" as could reasonably be expected. It is to be hoped that nobody will think that the methods described and recommended are particularly "simple" in practice. An interesting and appreciative foreword is contributed by Dr William Brown.

The Development of the Sexual Impulses. By R. E. MONEY-KYRLE. London: Kegan Paul, Trench, Trubner & Co., Ltd. 1932. International Library of Psychology, etc. Pp. ix + 219. 10*s*. 6*d*. net.

Dr Money-Kyrle's discussion contains far more than is indicated in his title. This is evident from the chapter headings which are: The Nature of Psychology; The Impulses of the Organism; The Phylogenesis of Impulses; The Development of Cultural Impulses; The Ontogenesis of Impulses, and The Effects and Value of Psychology. Nothing very new emerges from the first two chapters. Psychology is said to study the causal series, stimulus—cerebral process—reaction, and since it is as much, or more, interested in the *correlation* series cerebral processes—consciousness, the middle term of the three may always be equally well put in conscious terms. The argument seems to be formal and logical. It is the study of these interpolated conscious processes which leads to the consideration of organic "impulses" which occupies the second chapter. The remainder of the book is concerned with a study of the development of the specific impulses of sex in the race and in the individual, in a biological and in a cultural sense. The point of view is that of a convinced psychoanalyst. The study itself is clear, careful, very well written, unexaggerated and excellent of its kind. Within its limits it is difficult to see that more could have been done, though a critical reader will often wish for greater detail and more rigorous proof. The final chapter, which very admirably states the bearing of psychoanalytic doctrine upon religion, morals and social philosophy will be of great interest to every thoughtful reader. To most people the general issue will appear to be extremely pessimistic. "Desires," says the author in his last sentence, "are also satisfied when they are destroyed." But it may be that this is a formal and empty kind of satisfaction, far removed from anything upon which a normal human being could set much value.

Nevertheless this is a very good book.

Sex Hostility in Marriage. By TH. H. VAN DE VELDE. London: William Heinemann, Medical Books, Ltd. 1931. Pp. xvi + 296. 17*s.* 6*d.* net.

It is difficult to evaluate this book. The style of the writing will inevitably antagonize many readers for it is often unnecessarily sentimental, almost always extremely discursive and often much more indirect than it need be. Further, the author, in spite of a great amount of knowledge of his subject, is often rather out of his depth on the psychological side, and not quite equipped with the necessary background to make him a judicious critic of many rather ill-founded current views. Yet again some of the writing, especially on the psychological side, when doctrines of "types" come on the field is over-technical for the general reader. In spite of all this the book should do a lot of good, especially if it is read, as is intended, together with its companion books, *Ideal Marriage* and *Fertility and Sterility in Marriage. Sex Hostility* deals with topics that are of profound importance and are in general little understood and little discussed. Though the discussions here are longer than they need be, they should bring enlightenment to many, and may set a few upon lines of original investigation that are greatly needed. They cover a very large variety of different topics, but throughout a practical aim is kept closely in view: to teach the avoidance, as far as possible, of the disasters that may follow sex hostility in marriage.

The translation by Hamilton Marr is competent.

The Sex Factor in Marriage. By HELENA WRIGHT. London: Noel Douglas. 1930. Pp. 100. 3*s.* 6*d.* net.

In every possible way this is an extraordinarily good and useful little book. It is concise, clear, untechnical, it avoids all the sentimentality which often disfigures books on this kind of topic, and it gives exactly the sort of information which the ordinary young married person needs but usually cannot get. It contains no pretentious "psychology" but what is said about the mental aspects of sex in marriage is thoroughly sound. Nothing is shirked and everything is put straightforwardly and in a direct manner. In fact it is difficult to see how the author's purpose could possibly have been better achieved within the limits of space that she has set herself. Dr Wright is to be congratulated upon a very excellent and valuable piece of work and her book should be very widely read by those to whom it is addressed.

Discovering Ourselves: A view of the Human Mind and how it works. By E. A. STREAKER and K. E. APPEL. London: Chapman & Hall, Ltd. 1932. Pp. ix + 306. 15*s.* net.

"This book," say the authors, "is meant to be entirely practical. It is not written for psychologists or psychiatrists. It does not aim to discuss theoretical questions." In fact it tries to deal with many common problems of conduct in an excessively popular manner. Like many other such books it may be of some use to the general reader, and it is certainly, if its many diagrams are excepted, easily intelligible. But, also like many similar books, it has a way of making the correction of undesirable conduct appear a lot simpler than it really is. Taken "by and large" as the authors might say, it seems as if quite sufficient books of this type have now been published.

Psychology at Work. Edited by PAUL S. ACHILLES. London: McGraw Hill Publishing Co., Ltd. 1932. Pp. xiii + 260. 10*s*. 6*d*. net.

Here are a series of essays or lectures by seven American psychologists: (1) Psychology and the pre-School Child, by L. H. Meek; (2) The Study and Guidance of Infant Behaviour, by Arnold Gesell; (3) Psychology and Education, by Arthur J. Gates; (4) The Foundations of Personality, by M. A. May; (5) Psychology and the Professions, Medicine, Law and Theology, by W. R. Miles; (6) Psychology and Industry, by M. S. Viteles; (7) Psychology in Relation to Social and Political Problems, by F. H. Allport. The essays are naturally of variable value but they are all worth reading and some of them are very interesting indeed. The book should have a considerable appeal to the general reader, and will also be of use to the student.

The Province of Logic (an interpretation of certain parts of Cook Wilson's "Statement and Inference"). By RICHARD ROBINSON. London: George Routledge & Sons, Ltd. 1931. 10*s*. 6*d*.

To the student of philosophy this book is valuable because it is a very lucid exposition of Cook Wilson's approach to the problems and the field of logic. But there is little in it to attract the experimental psychologist. According to Cook Wilson, logic is the consideration of the nature of thinking about thinking. Hence the methods most characteristic of psychology are not applicable to it. As long as psychology confines itself to the experimental methods it now uses, it cannot include the study of the nature of thinking, since thinking about thinking is not discovery but recognition; and discovery is *a priori*. Contrary to accepted philosophical doctrine, however, Cook Wilson asserts that thinking is impossible without perception or imagination. It is a pleasure to read Robinson's beautifully lucid development and exposition of this thesis, and a relief to the psychologist to find that, after all, "psychology is practically necessary to logic."

The Emergence of Life: being a Treatise on Mathematical Philosophy and Symbolic Logic by which a new Theory of Space and Time is evolved. By JOHN BUTLER BURKE. Oxford University Press. 1931. Pp. ix + 396. 30*s*. net.

The Mystery of Life. By JOHN BUTLER BURKE. London: Elkin Mathews: The Library of New Ideas. 1931. Pp. 159. 3*s*. 6*d*. net.

A Philosophy in Outline. By E. S. BENNETT. London: Kegan Paul, Trench, Trubner & Co. 1931. Psyche Miniatures General Series. Pp. 160. 2*s*. 6*d*. net.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETINGS.

March 3, 1932. Discussion Meeting: "Conceptual Thinking." Opened by Mr A. W. Wolters, M.A.

SECTIONAL MEETINGS.

Medical.

March 22, 1932. "Some Reflections on Freud's 'Beyond the Pleasure Principle.'" By Mr Norman J. Symons.

April 20, 1932. Symposium: "The Psychology of Crime." Speakers: Sir William Clarke Hall, Dr T. Christie, Dr T. S. Good, Dearing Thomas, Esq.

May 25, 1932. Resumed discussion on: "The Psychology of Crime."

Education.

March 7, 1932. "The Use of Intelligence Tests in Selecting Children for Secondary Schools." By Dr A. G. Hughes.

May 2, 1932. "Temperamental Tests and Perseveration." By Dr R. B. Cattell.

Industrial.

May 11, 1932. "Some Observations on the Psychology of Sickness Absenteeism." By Dr May Smith.

SCOTTISH BRANCH.

November 7, 1931. "The Sense of Movement in Music." By Prof. D. F. Tovey.

„ „ "Ten Cases illustrating two Aspects of Clinic Treatment." By Dr M. MacTaggart.

December 5, 1931. "Individual Differences in Perception." By Dr R. H. Thouless.

„ „ "Number Forms." By Mr Morton.

January 30, 1932. "An Experiment with the Meier-Seashore Art Judgment Test." By Dr E. H. Connell.

„ „ "Some Notes on the Psychology of Prodigious Mental Calculation." By Dr J. D. Sutherland. With Demonstrations by Dr A. C. Aitken (Mathematics Department, Edinburgh).

„ „ "Personality Outline." By Dr E. W. Westburgh.

March 5, 1932. "Character and its Estimation." By Mr J. Wedeck.

„ „ "Demonstration of Dr Houstoun's Apparatus for measuring Brightness Thresholds." By Dr R. H. Thouless.

„ „ "Belgian Observation Centres: their Aims and Methods." By Miss F. Kennedy.

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BRITISH PSYCHOLOGICAL SOCIETY

RULES

NAME AND OBJECTS.

1. This Society shall be called the British Psychological Society.

2. The Society is instituted for promoting the co-operation of those interested in the various branches of Psychology.

3. The Society shall be responsible for the publication of *The British Journal of Psychology.*

MEMBERS AND SECTIONS.

4. The Society shall consist of Honorary Members, Ordinary Members and Sectional Associates.

5. Persons of scientific distinction who have contributed to the advancement of Psychology shall be eligible for election as Honorary Members.

6. Honorary Members shall be entitled to all rights and privileges of membership.

7. Honorary Members may be elected from time to time on the nomination of the Council at any General Meeting of the Society.

8. The Council shall be empowered to institute Sections of the Society, each Section being concerned with a special branch or aspect of Psychology.

9. Election to a Section or Sections shall *ipso facto* constitute election to the Society as a whole and entitle those so elected to receive notice of and to take part in all General Meetings of the Society in addition to the meetings of the Section or Sections to which they belong.

10. Sections shall have the right of electing Sectional Associates who shall receive notice of and shall be permitted to attend all Meetings of the Section or Sections to which they belong. Such Sectional Associates shall however take no part in the Government of the Society or the Sections, and they shall not receive *The British Journal of Psychology.*

10 *a*. Transfer from Sectional Membership to Sectional Associateship may take place on January 1st only, and after not less than six months' notice from the Member of his wish to transfer.

11. The procedure as regards the election of Sectional Associates shall be the same as that in the case of Members.

12. Every Candidate for election to the Society shall be recommended by at least two Members who shall be prepared to furnish information as to the Candidate's qualifications for membership. No Candidate shall be submitted for election to the Society who has not first been approved by the Council or by a Sectional Committee.

13. A Candidate who has been approved by the Council or by a Sectional Committee shall be nominated for election at the next General Meeting of the Society, or at the next Meeting of the Section respectively.

14. One full week before the Meeting at which the ballot is to take place, the Secretary shall send to each Member a balloting paper containing the names and addresses of the Candidates and the names of their proposers.

15. Any Member unable to attend a Meeting at which a ballot is to take place can vote by sending his balloting paper to the Secretary.

16. One adverse vote in five shall exclude.

17. To vote *for* a Candidate, a Member must prefix a cross to the Candidate's name; to vote *against* a Candidate, he must erase the Candidate's name. Members who wish to record a vote neither for nor against a Candidate must leave the name untouched.

18. Any Member whose subscription remains unpaid for two years may, after receiving due notice, be removed by a resolution of the Council from membership of the Society.

19. Whenever it shall be proposed to remove a Member from the Society for any reason other than failure in payment of subscription, the matter shall be brought as a formal resolution before the Council or one of the Sectional Committees (whichever it shall most concern). In the event of the resolution being passed, a ballot shall be held for the removal of the said Member at the next General Meeting of the Society, or at the next Meeting of the Section respectively. Should two-thirds of the Members voting vote in favour of the removal of the Member, that Member shall be removed from the Society.

20. If at any time it is desired to abolish any Section or Sections, to subdivide any Section or Sections, or to amalgamate two or more Sections, a Special Meeting of the Section or Sections concerned may be called for this purpose according to the conditions laid down in Rule 44. Resolutions passed at any such Meeting shall be reported by the Sectional Secretary or Secretaries concerned to the next Meeting of the Council. Should the Council approve of such resolutions, such resolutions shall immediately take effect. But should the Council formally express dis-

approval of any or all of the said resolutions, these resolutions shall be considered by an Extraordinary General Meeting of the Society summoned in accordance with the conditions laid down in Rule 44 and the decision of this Meeting shall be final.

In the case of the desire to abolish any Section or Sections, the following alternative procedure may be followed. An Extraordinary General Meeting of the Society shall be summoned in accordance with the conditions laid down in Rule 44, and any resolution passed at this Meeting regarding the abolition of the said Section or Sections shall be final.

OFFICERS, COUNCIL AND SECTIONAL COMMITTEES.

21. The business of the Society as a whole shall be conducted by a Council consisting of the President, Vice-Presidents, Treasurer, General Secretary, Librarian, Editors of *The British Journal of Psychology*, one representative each of every Section and Branch to be nominated by the Sectional and Branch Committees, one representative of the Committee for Research in Education to be nominated by that Committee, and of six Ordinary Members.

22. The President of the Society shall hold office for three years and shall not be eligible for re-election until after the lapse of a further period of six years. Every President shall at the termination of his period of office become a Vice-President for a period of six years. The other officers of the Society shall retire annually but shall be eligible for re-election.

23. Every year two Ordinary Members of the Council, having served for three years, shall retire and shall be ineligible for re-election until after the lapse of one year.

24. Names of Members to serve on the Council may be suggested by the Council or by any Member by sending the proposed names to the General Secretary at least two weeks before the Annual General Meeting.

24 *a. Nominations of representatives of the Sections, the Branches and the Committee for Research in Education shall be sent to the General Secretary at least two weeks before the Annual General Meeting.*

25. Nominations for the election of Officers shall be handed in to the General Secretary before the Meeting preceding the Annual General Meeting and be announced at the Meeting preceding the Annual General Meeting.

26. One full week before the Annual General Meeting, the General Secretary shall send to each Member a balloting list containing: (1) The names of the Council; (2) The number of the attendance of members of

the Council at the Council Meetings held during the year; (3) The names of those proposed by the Council *and Committees* or by Ordinary Members to fill the vacancies.

27. The Society may fill all the vacancies among the officers that may occur between two Annual General Meetings by ballot, in accordance with Rule 15, at the next General Meeting, due notice having been given. Similar vacancies among the Ordinary Members of the Council shall be filled by co-option by the Council.

28. In the absence of an ex-officio Chairman (in accordance with the conditions laid down in Rule 42) the Council shall elect one of its Members to act as Chairman of Council. A quorum at a Council Meeting shall consist of five. If there be no quorum, the Members attending shall have power to transact business, subject to the approval of absent Members, to whom the resolutions adopted shall be forwarded.

29. The first Meeting of each Special Section shall be convened by the General Secretary of the Society. At this Meeting the Section shall appoint its Chairman, Secretary and Sectional Committee.

30. The Chairman and Secretary of a Section shall retire annually but shall be eligible for re-election.

31. The size of a Sectional Committee shall be determined by the Section. A quorum at a Meeting of a Sectional Committee shall be as that Committee shall decide.

32. When Sections have been formed, Rules 12, 13, 14, 15, 23, 24, 25, 26, 27, 28, 40, 41, 42, 43, 44 shall (*mutatis mutandis* and unless expressly stated to the contrary) apply respectively to the Committees, Chairmen and Secretaries appointed by these Sections.

33. The Society shall not make any dividend gift, division or bonus in money unto or between any of its Members.

SUBSCRIPTIONS AND FINANCE.

34. Each Ordinary Member shall pay an annual subscription of one guinea, which shall entitle him to receive a copy of *The British Journal of Psychology*, with the exception of such additional portions of the *Journal* as are devoted to special branches or aspects of Psychology and are published separately.

34 *a*. Ordinary Members who are members of all Sections shall pay an inclusive annual subscription of £3. 2*s*.

35. In addition to the Ordinary Membership Subscription Members shall pay an additional fee for every Section which they join. This

additional fee shall entitle Members of any Section to receive such additional and separately published portions of *The British Journal of Psychology* as may be devoted to the special branch or aspect of Psychology studied by the Section. The amount of such additional fee shall be fixed by the Committee of the Section concerned in consultation with the Treasurer, and any resolution passed by the Sectional Committee with reference to such fee shall, before coming into force, receive the approval of the Council.

36. Sectional Associates shall pay a subscription the amount of which shall be determined by the Sectional Committee in consultation with the Treasurer of the Society.

37. A Member may compound for his annual subscription by a fee, the amount of which shall be determined by the Council after consultation with the Sectional Committee and the Treasurer. Such composition shall entitle the Member to all the privileges of membership, which he enjoys at the time of composition, for life.

38. The subscription is payable in advance and becomes due on January 1st. *The British Journal of Psychology* shall be sent only to Members whose subscriptions have been duly paid.

39. The Council shall grant to each Section permission to incur such expenses as may be necessary for the work of the Section; the approximate amount of such expenses to be determined by the Council.

MEETINGS.

40. The Annual General Meeting shall be held in December, when the General Secretary's Report and the Treasurer's Statement of Accounts shall be laid before the Society.

41. The General Meetings of the Society shall be at such times and places as the Council shall decide, but in each year there shall be at least four meetings.

42. The President, or, failing him, a Vice-President, shall, whenever possible, act as Chairman at the Meetings of the Council and of the Society. In the absence of the President or a Vice-President, some other Member shall, on the motion of the General Secretary or his substitute, be elected to act as Chairman.

43. The Secretary shall send to all Members of the Society notices of each Meeting at least seven days beforehand, and of the business to be transacted at the Meeting.

44. An Extraordinary General Meeting may be summoned at any time by the Council, or by the General Secretary on the written request of twenty Members; at least fourteen days' notice of such Meeting shall be sent to each Member of the Society.

VISITORS.

45. Each Member shall have the privilege of introducing two visitors at any Meeting. The Council or a Sectional Committee shall however have the power of closing any Meeting or Meetings to visitors, provided notice to this effect be given on the notice convening such Meeting or Meetings. The names of visitors together with the names of Members introducing them shall be entered in a book provided for that purpose. Any Member of the Society attending a Meeting of a Section to which he does not belong must be introduced by a Member of that Section.

BRANCHES.

46. The Council shall be empowered to sanction and dissolve the establishment of Branches of the Society throughout the British Empire. Members of such Branches shall pay the same subscription as that of Ordinary Members direct to the Treasurer of the Society and be entitled to the same rights and subject to the same rules as Ordinary Members. The Council shall be empowered to refund to the Treasurer of any Branch such proportion of Branch Members' subscriptions as the Council may determine to meet local expenses.

46 *a*. Any Rules made by Branches of the Society shall be first submitted to the Council for their approval.

46 *b*. The minimum number of Members necessary to form a Branch shall be 10.

46 *c*. Branches shall be required to present a report to each Annual General Meeting.

RULES.

47. The Rules of the Society may be altered at the Annual General Meeting, after notice of alteration has been given at a previous Ordinary General Meeting, or they may be altered at an Extraordinary General Meeting summoned for that purpose. Such Extraordinary General Meeting shall be held in London, and the purpose for which it is called shall be expressly stated in the notice convening the Meeting. No other business shall be transacted thereat.

VOLUME XXIII OCTOBER, 1932 PART 2

SOME PSYCHOLOGICAL EXPERIMENTS ON MENTAL DEFECTIVES IN RELATION TO THE PERCEPTUAL CONFIGURATIONS WHICH MAY UNDERLIE SPEECH. II.

BY R. G. GORDON AND R. M. NORMAN.

(*From the Stoke Park Colony for Mental Defectives, Bristol.*)

IV. *Further illustration of manipulation of spatial configuration afforded by Puzzle Box experiments with mental defectives* (pp. 85–103).
  - (1) *Introductory remarks* (pp. 85–86).
  - (2) *The material and method of procedure* (pp. 86–91).
  - (3) *Results. Class A* (pp. 91–95).
  - (4) *Results. Class B* (pp. 95–99).
  - (5) *Discussion and conclusions* (pp. 99–103).
    - (i) *Consideration of the problem involved* (pp. 99–102).
    - (ii) *Consideration of the time tables* (p. 102).
    - (iii) *General conclusions* (pp. 102–103).

V. *Blot tests* (pp. 103–112).
  - (1) *Preliminary observations* (p. 103).
  - (2) *Method of experiment* (pp. 104–105).
  - (3) *Results* (pp. 106–111).
    - (i) *Goodness of imagery* (pp. 106–107).
    - (ii) *Varieties of images* (pp. 108–111).
  - (4) *Conclusions* (pp. 111–112).

References (pp. 112–113).

IV. FURTHER ILLUSTRATION OF MANIPULATION OF SPATIAL CONFIGURATION AFFORDED BY PUZZLE BOX EXPERIMENTS WITH MENTAL DEFECTIVES.

(1) *Introductory remarks.*

THE use of puzzle boxes as a means of investigating adaptive behaviour in animals has been used extensively since its introduction by Thorndike in 1899(1). Since then the method has been applied to birds, rats, squirrels, cats, dogs, racoons, porcupines and monkeys. The experiments have been of two main types. In the first, which has been used mainly with the smaller animals, the subject has been confined within a cage from which he has escaped to liberty and food only after successfully manipulating some mechanical device securing the door, for example, by pulling a string or shifting a lever. In the other type of experiment, the subject is outside the box in which food is visible and which can be opened by moving similar mechanisms this time on the outside. This method has been chiefly used with monkeys by Thorndike (2), Kinnaman (3), Cole (4), Hobhouse (5) and Buytendijk (6) and others, and it is the method used in this investigation. Much criticism has been levelled against this method on the grounds that the problem is of too mechanical a nature, and that the animal could not be expected to understand what he was about. This criticism undoubtedly has force in some of the complicated combination lock systems (for example, as used by Kinnaman) especially where some part of the mechanism is out of sight. In the series of boxes employed in the present experiments a much greater simplicity has been maintained, and in this respect they resemble those employed by Buytendijk with his chimpanzee Sim.

It was proposed to investigate by this method:

(1) How the behaviour of mental defectives compares with that of lower animals, especially the anthropoids, in their appreciation and manipulation of spatial configurations.

(2) What relation exists, if any, between ability to succeed in such tests and general intelligence as measured by the Binet-Porteus combined Intelligence Quotient (7), (8).

(3) The value of the time taken during the test as a criterion of intelligence, *i.e.* is this method of any diagnostic value when applied to human mental defectives as a test of general intelligence?

(2) *The material and method of procedure.*

Five puzzle boxes were designed, each of the same shape and size and differing only in the complexity of the lever systems which prevented the lid from being opened. Each box was approximately cubical in shape (lid 9½ × 8½ in., height 7½ in. with lid closed) and were constructed out of ¾ in. wood. The lid when closed was flush with the sides of the box. Two panes of thick glass were set in the roof and front wall respectively, so that a clear view of the interior was easily obtained. The exterior of the box and the levers were stained dark brown, the interior being left unstained.

Box I had no clasp and the lid had only to be raised (Fig. 4).

Box II had a single lever (Lever 1) at its left-hand top corner, pivoted to the front of the box, and at its upper end bent at a right angle so that

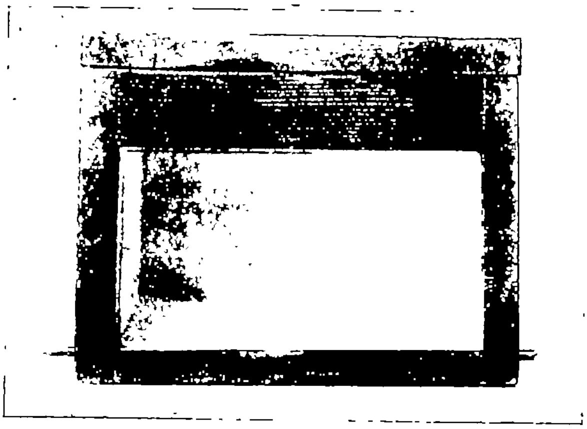

Fig. 4.

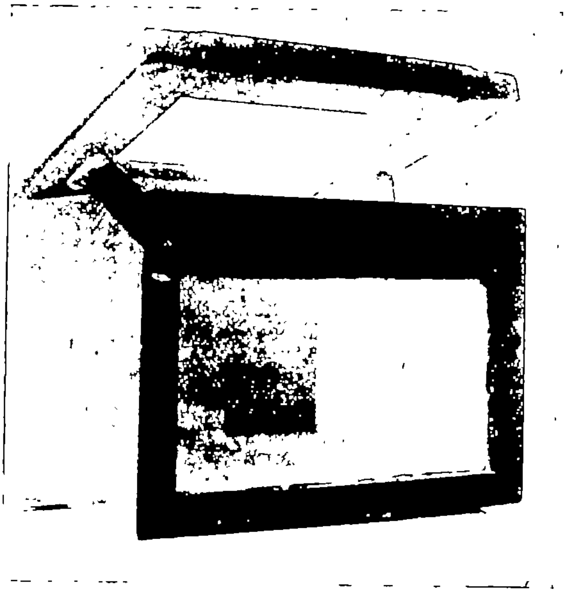

Fig. 5.

when vertical it prevented the opening of the lid. A lateral movement of the lever freed the lid (Fig. 5).

Box III had a somewhat similar lever, pivoted however to the left-hand side of the box, so that a forward movement freed the lid. In addition, there was a second lever, placed symmetrically at the right-hand side. In the subsequent account these levers will be referred to as No. 1 and No. 2 respectively (Fig. 6).

Box IV. This was similar to Box II with the addition of a laterally placed lever (No. 1) pivoted to the left side of the box, which had to be rotated either anteriorly or posteriorly before Lever No. 2 could be moved (Fig. 7).

Box V. This resembled Box IV, but this time the lower part of the laterally moving lever (No. 2) was obstructed by a horizontally placed lever (No. 3) pivoted on the front of the box above the glass panel, its free end being in contact with the vertical part of Lever No. 2, thus preventing the latter's movement to the left (Figs. 8 and 9).

It will be seen that these boxes presented a series of increasing difficulty and that Box V is one lever more complicated than Box IV. But Boxes III and IV, while each was one degree more complicated than Box II, bore no direct relationship to each other.

A chocolate was placed in each box to act as bait, the lid closed, and the respective levers adjusted. As the interior of the box was unstained, the bait was clearly visible.

The first box in the series (Box I) was fastened by thumbscrews to a heavy board which was placed on an ordinary table. The subject was then introduced into the room, was given a chocolate to eat, and was led up to the box so as to face its anterior surface. He was then told to open the box and get the chocolate which was pointed out to him through the glass. Nothing was said about a time limit or hurrying. A stop-watch was started and the time was taken from the first movements till the lid had been successfully opened. The observer retired into the background so as not to interfere with the subject, or embarrass him, so far as was compatible with accurate observation. At the conclusion of the test the order in which the levers were moved, the direction of the attempted movement, and where possible the number of errors (*i.e.* unnecessary movements) made, were recorded. After Box I had been opened, Box II was substituted, and so on through the series. In the event of a failure to open the box as evinced by a complete cessation of striving in spite of encouragement, the method was shown to the subject, who was allowed to proceed with the series after he had mastered the difficulty.

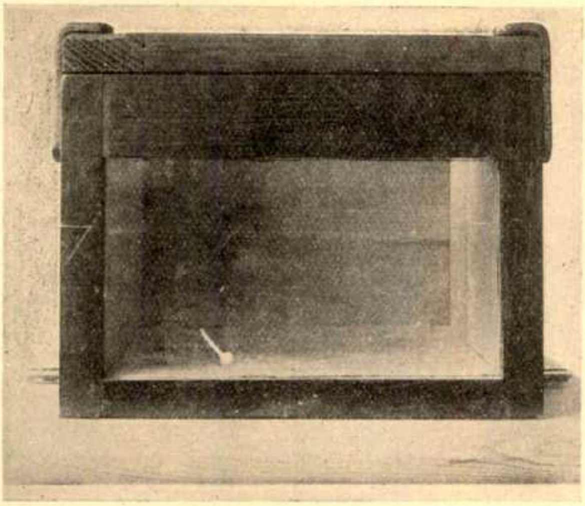

Fig. 6.

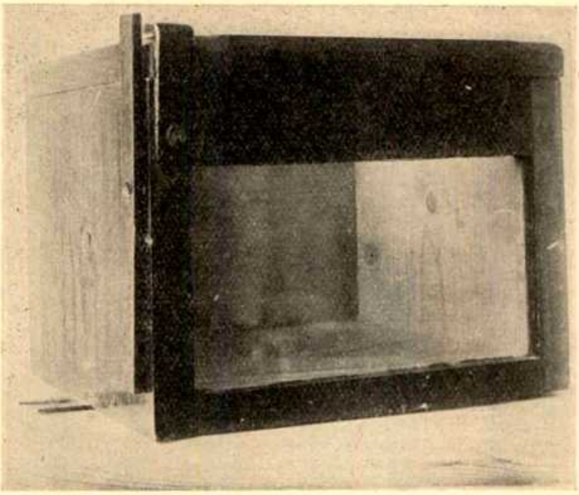

Fig. 7.

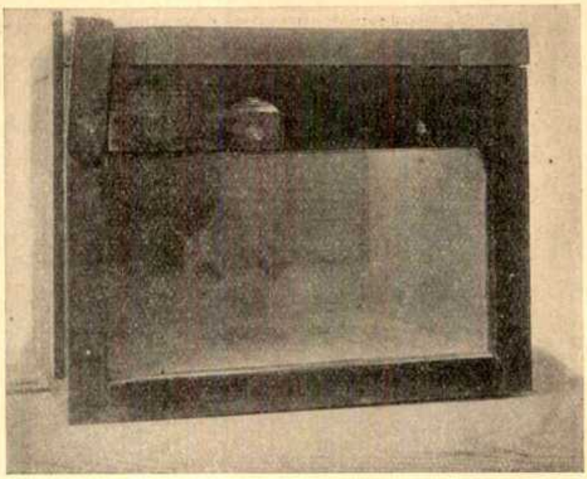

Fig. 8.

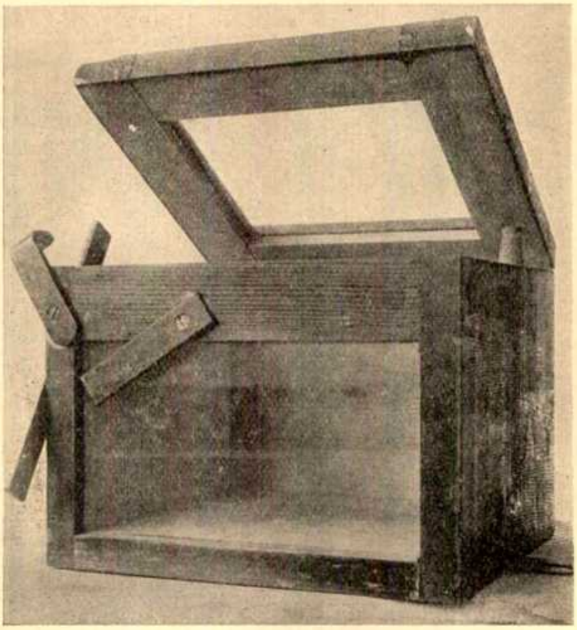

Fig. 9.

It was hoped that owing to this comparative simplicity of the task it would be easy to analyse the behaviour displayed, since the number of errors made might be expected to be correspondingly few. In using mentally defective persons for these experiments, the charge of anthropomorphizing the subject when an attempt to interpret their behaviour is made, loses any force it may have in the case of apes. Yerkes (9) has described the behaviour of Köhler's chimpanzees (10) as 'almost human,' and if the human behaviour to be described subsequently may in many cases be described as 'almost Simian,' the tables for once will have been turned on *Homo sapiens* and the gulf between the two species still further filled in.

The two distinct classes of mentally defective persons have been used in this investigation which were used in the first two groups of tests. The high-grade cases were tried with the fifth and hardest box only, as it was found that they learnt without any difficulty from the series, so that the graduated test was of no value. The low-grade cases were tested with the whole series of boxes.

It has not been practicable to try out Box V on more than a few intelligent adults, but those who tried to open it found the task of almost ridiculous simplicity, and no one has taken more than 15 sec. in the process. The observations of other authorities on the capacity of normal persons to solve analogous problems makes it unnecessary to repeat such experiments. In normals it was unusual for any superfluous movement to be made—at most one movement of Lever No. 2 against 3 is sufficient to ensure the detection of the latter and its movement. Usually, however, even this is unnecessary, the pattern of the levers being grasped at a glance, while the subject walks towards the box.

(3) *Results. Class A.*

In the following analysis of the behaviour exhibited in the first class of mentally defective persons, Class A, any superfluous movement is considered to be an error.

It was possible to distinguish three main types of response (see Table I).

Table I. *Class A.*

Type I.	Good response, 33 per cent.
	(*a*) No error, 13.
	(*b*) One error, 6.
	(*c*) Two errors, 14.
Type II.	Trial and error, 39 per cent.
	(*a*) Response by exploration and elimination, 17.
	(*b*) Response by random movements, 18.
	(*c*) Lid not opened though free, 4.
Type III.	Forcing response, 28 per cent.

Type I. 33 *per cent.*

'A good response,' in which the box was opened with no error (*i.e.* with only four movements including moving the lid), one error (*e.g.* clicking one lever against another, or trying the lid while it was obstructed, or moving a lever twice), or two errors.

It is only in this group (Type I) that the time taken is of any accurate indication of the merit of the response.

(*a*) No errors, 13 per cent. The time taken was under 15 sec. in every case but one. The exception was due merely to slower movements and a rather exaggeratedly methodical method.

Example (L. M., aet. 33, I.Q. 81·5). Time 8 sec. "Lever 1 moved. Lever 3 moved down. Lever 2 moved and the box opened. A 'balanced' co-ordination of movements in which each contributed its proper share in the attainment of the goal."

(*b*) One error, 6 per cent. Four persons tried to move No. 2 lever while it was obstructed by one of the others. This clue was sufficient for them to open the box without further error. The remaining two tried the lid while it was obstructed.

Example (B. W., aet. 25, I.Q. 75). Time 10 sec. "Lever No. 1 moved down at once. No. 3 then moved deliberately up. No. 2 was then incorrectly moved inwards to the right and immediately afterwards correctly outwards to the left and the box opened."

(*c*) Two errors, 14 per cent.

Example (A. B., aet. 37, I.Q. 44·5). Time 19 sec. "No. 2 made to click against No. 1. No. 1 moved with left hand. No. 3 then depressed. Lever No. 2 moved inwards (error) then correctly outwards. Lid opened at once."

A type of error occasionally observed was the moving of a lever, replacing it and moving it again, as if indulging in a preliminary try out.

Type II. Trial and error, 39 *per cent.*

(*a*) Comparatively intelligent response, 17 per cent. This response consisted in exploration and elimination and insensibly emerges from Type I (*c*), only differing from it in the number of errors made in the exploratory phase. The response is considered better than that occurring in subdivision (*b*) because the recognition and elimination of errors was a more marked feature. Tactile exploration and the 'trying out' of the various movements seemed to be more frequently adopted than the visual analysis chiefly used in Type I. It was not uncommon to see subjects looking about the room as they moved the levers. The characteristic method employed was that the levers were moved against one another, the sense of obstruction providing the clue to the next movement. Often the lid would be tried after a lever had been moved; this failing, the next lever would be moved, the lid again attempted and so on.

Example (R. F., aet. 21, I.Q. 59). Time 44 sec. "Slow and deliberate actions. Inspection. Lever 3 moved. Lid tried. Lever 3 moved with right hand. Lid tried. Inspection and further movement of Lever 3. Then No. 2 moved with left hand and lid opened."

One of this group took 175 sec., the length of time being solely due to the extraordinarily slow and dreamy movements—not to their multiplicity—another example of the fallacy of judging a response by time alone.

(*b*) Random movements, 18 per cent. This type of response most resembles that described by Thorndike (1) as characteristic of his cats and dogs. The solution appears to the observer to have occurred by chance acting through a multiplicity of movements repeated without profit and with little sign of insight.

In Bühler's phrases (11), there is an 'over-production' of movements and an 'aimless trying-out.'

An extreme example is the following:

(E. E., aet. 19, I.Q. 63.) Time 4 min. 34 sec. "Left hand used throughout. Lever 1 rotated several times. Then left thumbscrew touched. Lever 3 was moved about several times and replaced. Lever 1 moved again. Lever 2 was then clicked against 3 four times. These various movements were repeated for some time and at the end of each bout of trying the levers were usually in their original places. Twice when Lever 1 was moved, Lever 2 was clicked against 3. Eventually this relationship was seen (apparently), as Lever 3 was moved, and 2 cautiously moved about 2 cm. A short pause and then the lid was opened. Subject then laughed in a surprised way."

(*c*) In four cases a peculiar response was observed. After the levers had been moved, often with some degree of insight, the lid was not opened, but renewed lever movements occurred, and the box only opened later. In one case the subject gave up, saying "I can't do it," and had to be told to raise the lid. This 'stupid error' (Köhler (12)) seems to be analogous to the behaviour reported by Adams (13), who repeated some of Thorndike's experiments (1) with cats. In these, the cats continued striking at the levers, after successfully having opened the cage door, and did not emerge at once to secure their food.

Example (A. A., aet. 20, I.Q. 45). Time 58 sec. "Lever 3 moved down at once to 180°. Lever 1 was then rotated several times and Lever 2 was moved correctly. Instead of opening the lid, the subject reverted to the rotation of Lever 1, which was rapid and apparently conducted for its own sake. Lever 2 being moved back to its original position so as not to interfere with the rotatory movement. After some time, Lever 2 was moved away again and the box opened, that being the first time an attempt has been made to open the lid."

It is possible that had the chocolate been larger or more tempting the goal would not have been forgotten in this way. Köhler (14) found that he could sometimes obtain better results from the chimpanzees by reinforcing the customary bunch of bananas with some pieces of orange.

Type III. Forcing response, 28 *per cent.*

Although this type of response might also with justice be described as a method of trial and error, it differs in one particular from that described under Type II. This difference lies in the fact that remarkable difficulty was experienced in appreciating the significance of Lever No. 3, Lever No. 2 being invariably forced against it persistently without an attempt being made to move it, or even to 'look' for it. In six cases sheer force prevailed and the lid was squeezed past the still partly obstructed Lever No. 2. This was admittedly due to a defect in the apparatus, but to have prevented the possibility of this type of solution would have entailed too tight an adjustment of the levers, which would have interfered with a more rational solution. In two cases the attempt was abandoned and success only came after the observer had encouraged the subject. In two cases actual help had to be given; thus the observer asked "What's preventing it?" In one case the solution followed immediately; in the other the subject had to be told to look for the obstruction. A very similar result was obtained by Buytendijk (15) with his chimpanzee Sim. Here a simple lever was obstructed by a weight which had to be moved away before the lever could be moved and the box opened. In spite of the fact that the ape understood the use of the lever, he failed in every trial to remove the weight. This point will be returned to when the second class of mentally defectives, Class B, is considered. In one case the lid was not opened when the levers had been shown (as in Type II (*c*)).

In 22 per cent. then, Lever 3 was found and moved only after prolonged forcing. This forcing was often carried out in bouts between which the subject often vaguely moved Lever No. 1 to and fro or tried the lid. The forcing was usually conducted with the left hand, the subject putting her weight on Lever 2, while staring with a tense expression at some other part of the room. In twelve cases, during some such procedure, the right hand was observed to move in a fleeting manner towards the obstructing Lever No. 3. This was touched momentarily, but the hand was withdrawn after having accomplished nothing, or rarely after a slight movement, carried out with no conviction. Then the forcing would be continued as before, until at last the obstruction was 'looked for' and adequately moved. In seven cases this forcing response occurred simply as an incident in a trial and error procedure and these have been included in Type II.

The solution often came suddenly, apparently as a rush of insight as the nature of the obstruction was realized, and would seem to be strictly comparable to the 'jerk' or 'flash' quality described by the Gestalt psychologists as characteristic of the 'closure' by which a behaviour configuration comes to a satisfactory conclusion and tension is relieved. For this type of response Bühler (16) has coined the word 'Aha-erlebnis,' which admirably describes the surprise and the pleasure evinced.

Example (B. C., aet. 22, I.Q. 67). Time 2 min. 45 sec. "Lever 1 moved right round to its original position. Lever 2 moved so that it clicked against No. 1 which was then moved out of its way. The whole time was now spent in a desperate attempt to force Lever 2, in spite of warnings of the futility of the procedure (we feared for the apparatus). Suddenly, after a short discouraged pause, Lever 3 was moved down. Lever 2 was moved somewhat doubtfully and the box opened."

(4) *Results. Class B.*

The conditions of the experiment differed in this group in that the whole series of boxes was presented. It was anticipated that by this means the subject would be assisted not only by presenting the lever complexes in graded order of difficulty, but by sustaining his interest in the task by his rapid success in the earlier boxes. By the time that the final box was reached, therefore, given some learning capacity, the subject would be familiar with:

(1) The general notion of lever movement.

(2) The movement of opening the lid (not so simple as might be imagined, as the end did not project).

(3) An idea of where to find the levers in relation to the rest of the box—a 'place-analysis' in fact.

Table II shows a detailed report of the results. When a failure occurred (*i.e.* a cessation of striving, or persistent wandering of the attention in spite of exhortation), the difficulty was explained to the subject and he was then allowed to try the next box. This help sometimes took the form of touching the unobserved lever and showing the subject how to move it for himself, or if this failed (as it sometimes did), the movement was carried out before his eyes. In Class B this was effective in all but one case. The same result was obtained with monkeys by Kinnaman and Haggerty (17).

Table II. *Class B.*

- Group (i) Not able to open Box I, 17 per cent.
  - (*a*) Failed after being shown how, 9.
  - (*b*) Shown I, success in II only, 1.
  - (*c*) Shown I, success in II, III, IV, 1.
  - (*d*) Shown I and II, success in III and IV, 2.
  - (*e*) Shown I and II, failed III, 3.
  - (*f*) Shown I and II, success in III, IV, V, 1.
- Group (ii) Success in I, failed II, 17 per cent.
  - (*a*) Not allowed to proceed, 2*.
  - (*b*) Shown II, failed III, 1.
  - (*c*) Shown II, success in III only, 3.
  - (*d*) Shown II, success in III, IV, 7.
  - (*e*) Shown II, success in III, IV, V, 4.
- Group (iii) Success in I, II, failed III, 5 per cent.
  - (*a*) Not shown III, 3.
  - (*b*) Not shown but passed IV, failed V, 1.
  - (*c*) Shown III, success in IV and V, 1.
- Group (iv) Success in I, II, III, IV, failed V, 29 per cent.
  - (*a*) Forcing unsuccessful, 13.
  - (*b*) Forcing opened box but Lever 3 not touched, 2.
  - (*c*) Forcing opened box but Lever 3 not touched, 6, and on repetition failed to move Lever 3, 4.
  - (*d*) Lever 3 touched but not moved, 10.
- Group (v) Success in I, II, III, IV, V, 32 per cent.
  - (*a*) Trial and error, 6.
  - (*b*) Lever 3 discovered and moved after forcing, 20.
  - (*c*) Satisfactory response, 6.

* The behaviour was so primitive that further trial was deemed unnecessary; for example, in one case the subject fastened his teeth into the lid and made quite strong efforts against the obstructing lever.

Group (*i*). Unable to open Box I, 17 per cent. Nine out of the seventeen who failed to open Box I did not succeed in learning by imitation when shown. They did not take the chocolate even when the lid was wide open, nor did they repeat the movement after they had been passively 'put through' the action, to use Thorndike's expression (1). When, however, their hand was passively put into the box so as to touch the chocolate, they usually seized it with avidity and transferred it rapidly to their mouths, leaving little doubt that its nature had been recognized and appreciated. In this behaviour they resembled the dogs in the similar experiments of Hobhouse (5) which were confirmed by Buytendijk (6). These nine subjects were all of the lowest grade of idiot, and displayed not the slightest sign of comprehending what was required of them. They showed either a vague indifference or a rapidly shifting attention which could not be directed to the task in hand. Of the eight who mastered Box I (after being shown how to move the lid), only two succeeded with Box II, the others merely carried out repeatedly the movement they had just learned, that is, they kept on pulling at the lid.

Group (*ii*). Succeeded in opening Box I, failed in Box II, 17 per cent. Having mastered the principle of the first lever, when this had been demonstrated to them, only four (including three carried forward from Group (i) (*e*)) were unable to proceed further.

Group (*iii*). Succeeded in opening Boxes I and II, but failed in Box III, 5 per cent. These five who, having progressed up to and including Box II, were unable to open the very similar Box III, all gave low-grade responses showing little or no insight. Their success with Box II was due in most cases to random movement and the fortunate accident of displacing the lever gave them no clue to the analogous situation presented by Box III.

Example (E. C., mental age $2\frac{2}{12}$ by Merrill Palmer test).

Box I. "Slow approach. Opened the lid at first trial."

Box II. "Lid tried several times, the hands being passed round it. Eventually his hand touched the lever which he moved. Lid opened. 33 sec."

Box III. "Lid tried. Lever 1 attempted in a lateral direction (as in Box II) but not with sufficient force in an anterior direction to move it completely. Box moved about. Levers 1 and 2 moved a little in a correct direction. Subject gave up the attempt after 2 min. without displacing either lever entirely, despite persistent fingering."

Example (E. B., mental age $2\frac{3}{12}$ by Merrill Palmer test).

Box I. "Opened at once."

Box II. "25 sec. Lid tried several times. Lever moved accidentally by brushing against it."

Box III. "Failure after 3 min. Lever 1 was displaced accidentally in the course of prolonged tugging at the lid, but the other lever was not touched. Attention wandering all the time."

Group (*iv*). Success with Boxes I, II, III, IV, failed at V, 29 per cent. Box IV gave little difficulty and only three persons failed in it after having negotiated Box III.

In Group (iv), twenty-nine subjects were unable to locate and move Lever No. 4 in Box V. Six opened Box V by vigorous forcing in spite of warnings from the observer. In four of these cases the box was immediately re-set (out of sight of the subject) and the test repeated.

In spite of the fact that in the first trial the forcing powers had pushed Lever 3 violently upwards and had often actually come against the hand of the subject as he opened the lid, in the second trial the forcing was resumed and no notice taken of Lever 3. After a while this forcing was spontaneously abandoned, or was stopped by the observer when it became obvious that no new procedure was forthcoming.

In surveying the results obtained in the series of Boxes I–IV a perseverative tendency can be detected. This was shown throughout the series by the 'carrying-over' of a movement which had proved successful in one box to the attempt on the next. In Boxes II, III and IV the bent lever holding down the lid is so pivoted that the movement required to shift it alternates. In Box II a lateral movement is effective, in Box III a forward movement, in Box IV a lateral movement again. The first opportunity for a 'carrying-over' of this nature occurs in Box II where the lid was tried (before touching the lever), and this movement was usually repeated several times despite its futility. This occurred in 76 per cent. of cases. The perseverative effect of Box II in Box III was seen in 24 per cent. of cases. The perseverative effect of Box III on Box IV was seen in 27 per cent. of cases.

Group (*v*). Thirty-two subjects were eventually successful, *i.e.* they were able to work through the entire series without help of any kind. Only six used a pure method of trial and error without learning from experience, while twenty found Lever No. 3 after some forcing of Lever 2. Thus, in the two groups (iv) and (v) consisting of sixty-one persons in all, only 10 per cent. used the pure trial and error method. This can be explained by the fact that these subjects could learn and that in the previous boxes the lever holding down the lid (No. 1 in Boxes II and III, No. 2 in Box IV) had been successfully moved, and so in the majority of cases there was no delay in selecting it for trial with conviction. Moreover, the first two movements made in Box V (Lever 1 moved and No. 2 tried) were the very ones already successfully adopted in Box IV, so the perseverative tendency already described would reinforce the 'place analysis.' The difficulty lay in the blind perseverance with this forcing and in the ability to appreciate the new lever. The same repetition of unsuccessful movements has been observed by Ruger (18) who tested normal adults with wire puzzles.

Group (v) (*c*) contains the best type of response. The six cases in this group showed a very marked improvement on their fellows, so good in fact that the observer made certain in each case that it was not a second trial. It must be admitted that four of these boys, although technically imbeciles, were of higher grade and capable of manual work under supervision, so 'transfer of learning' may account for their success.

Table III. *Analysis of results in this subgroup.*

Errors made in Box V	Errors made in Boxes I, II, III, IV	Number of cases
0	3	1
1	3, 4, 5	3
2	7	1
3	5	1
	Total	6

In 39 per cent. of Class B (compared with 32 per cent. in Class A) some or all of the levers were replaced. The percentage may be larger in Class B for the simple reason that the chances of doing so were 4 : 1 in the case of those who had worked through the series. In Class A, the movements were usually so 'automatic' that it was thought they might be due to general training in neat and tidy behaviour. This may be true, but when the procedure occurred among the imbeciles a different explanation suggested itself. In this class the movements were often uncertain and slow and sometimes imperfect. Occasionally they were repeated. The impression was given that the subject wanted to make sure of this new trick he had learned. Sometimes this replacement of the levers occurred when the subject had been told to sit down after the box had been opened and while the report was being written, the subject often taking pains to replace the levers exactly. It would seem that this was done from sheer intellectual curiosity, and satisfaction over an accomplished task.

One is irresistibly reminded of the behaviour of Konsul, Köhler's most talented chimpanzee (19), who, when he had achieved the remarkable feat of fitting together two short sticks to make a longer one with which to fish in some fruit outside his cage, was observed to repeat this procedure with evident satisfaction, even when there was no fruit as a definite objective.

(5) *Discussion and conclusions.*

(i) *Consideration of the problem involved.*

The point must now be considered why Box V should have presented such difficulties to the vast majority of both classes—often unsurmountable difficulties to Class B. Box IV was easily solved. In both boxes the problem was superficially the same, that is, Lever 2 was obstructed by another lever (No. 1 in Box IV and No. 3 in Box V). The similarity ends here, for the positions of the obstructing levers are different and it is in this difference that the difficulty must lie.

In Class B it was the exception to find a co-operation between hand and eye. This tendency was also noted in Class A, but was more evident the lower the grade of the subject. Touch was therefore the main channel of information, and in Box IV the left hand naturally came into contact with Lever 1 when Lever 2 was moved (as had been learned in the previous boxes). Thus when Lever 2 clicked against Lever 1, there was no need for the sense of obstruction to give rise to an explicit idea "there is something obstructing this lever." The hand had already touched Lever 1, and by this time it was realised that a lever (cold and hard to the touch) was 'something to be moved.' In Box V, however, the obstructing lever was not at hand, it was somewhat remote from the area already known by 'place analysis' (the left-hand top corner). Moreover, it was usually out of sight if in their forcing efforts the subjects approached too near the box to see the front of it properly. Thus, in this case it was necessary for the sense of obstruction to give rise to an explicit idea of 'something obstructing' and therefore 'something to be moved away' in order to achieve success. That this process occurred in a subject as an implicit idea—without true insight—may be inferred perhaps from the transient touches the subject sometimes gave to Lever 3 which, inadequate in themselves, were often followed by true insight and subsequent movement of the lever. This process may be given a theoretical interpretation by invoking Spearman's third principle of cognition—the eduction of correlates (20). Thus, the given fundament is Lever No. 2, the relation is the sense of obstruction, and the correlate to be educed is the 'some-

Table IV. *Table to show the relationship between type of response, and time taken. Class A.*

Type I. (*a*)														
I.Q.	87·5	81·5	79	73·5	72	72	65·5	64	60·5	54	51	48	44	
Time in sec.	7	8	7	5	30	7	15	7	10	13	5	11	13	
(*b*)														
I.Q.	84	78	75	73	58·5	54								
Time in sec.	16	24	10	21	14	32								
(*c*)														
I.Q.	80·5	80	76	75	74	68	67	67	58	55·5	48	45	44·5	44·5
Time in sec.	26	29	33	30	15	19	20	27	19	17	39	23	19	20
Type II. (*a*)														
I.Q.	89	78·5	75	74	70·5	66·5	66	65·5	59					
Time in sec.	15	18	33	17	40	36	27	13	44					
I.Q.	59	58	58	54·5	54	50·5	49	40						
Time in sec.	175	34	54	120	20	22	32	18						
(*b*)														
I.Q.	73*	73*	67	65	63	60	58	58	57*	56				
Time in sec.	210	103	210	240	274	218	129	56	82	352				
I.Q.	55	50·5	49	48*	45·5*	45	45	44·5	40·5	40				
Time in sec.	76	125	86	12	102	42	120	56	80	45				
(*c*)														
I.Q.	85*	58	57	48·5	45									
Time in sec.	49	127	40	53	58									
Type III.														
I.Q.	76·5	75	72·5	71	69·5	67	64	63	63	63	61			
Time in sec.	94	119	91	135	145	165	115	83	16	224	205			
I.Q.	61	60	48	48	47	47	46	46	45·5	45	43			
Time in sec.	165	119	348	171	255	92	110	155	38	55	41			

* Mixed type of response, *i.e.* trial and error plus a forcing phase.

Table V. *Table showing the times taken (in sec.) in Groups (iv) and (v), Class B.*

Group (iv).

Box II	Box III	Box IV
20	8	18
5	15	17
8	5	10
3	9	5
20	11	6
18	7	18
16	14	8
18	23	30
22	11	105
29	10	9
No delay	No delay	No delay
47	10	9
52	4	5
8	3	6
38	6	93
14	4	9
7	2	4
57	21	43
9	11	7
29	13	16
11	5	8
36	6	8
17	13	10
4	138	4
17	14	8
3	11	3
40	6	4
7	14	10

Group (v).

Box II	Box III	Box IV	Box V
26	8	5	63
30	16	31	53
No delay	5	3	26
12	2	4	10
2	7	11	9
20	6	12	30
33	10	18	67
No delay	No delay	No delay	13
38	11	11	126
8	7	6	40
3	3	3	32
45	5	9	19
2	6	5	26
8	11	16	21
20	10	5	40
9	7	7	39
8	3	5	8
No delay	6	4	16
23	6	4	58
2	8	7	78
20	31	13	41
69	101	103	51
50	16	5	23
18	9	11	36
16	21	35	11
28	31	18	78
No delay	20	21	51
26	4	9	63
13	9	7	9
10	5	12	167
6	6	11	82
61	91	125	164
0	7	7	58

thing obstructing,' *i.e.* Lever No. 3. Or again, the whole process may be summed up in the language of the Gestalt psychologists by saying that the configuration of Levers 1 and 2 has expanded to include Lever 3.

(ii) *Consideration of the time tables.*

Class A. It will be seen that the time taken is short with Type I and markedly lengthened in the more primitive responses of Types II and III. Also, that the Intelligence Quotient bears no constant relationship to either time taken or type of response. Thus as a criterion of efficiency or mental grading, a performance test of this nature would appear to be of little general value. Whether, on the other hand, it is of value in distinguishing the feeble-minded as a class from the normal adult as a class, cannot be decided from the present investigation, though it is clear that a swift grasp of the problem and co-ordinated solutions does not occur readily amongst those of lower intelligence.

Class B. Generally speaking the time taken (in this case an index of learning) shows a marked fall from Box II to Box III, a slight rise in Box IV (as compared with III but not II) and a marked rise in the case of Box V which in Group (iv) was not successfully opened. This shows that there was a capacity for profiting by experience up to a point, but that in many cases the limit of this capacity was soon reached. A situation involving two or three factors could be mastered, but one involving four factors was too difficult.

(iii) *General conclusions.*

The conclusions that may be drawn from these experiments are as follows:

(1) The results show a definite similarity to those obtained by analogous experiments with animals, especially with the anthropoids. As many as 144 out of 200 did not rise above the general trial and error method reinforced by a capacity to learn, which is the way in which experimental animals tackle the problem; that is to say, they were unable to form an immediate perceptual configuration of the whole problem.

(2) With the exception of the lowest grade idiots, learning by experience was the rule, so much so that in Class A the test was valueless if the whole series was used.

(3) Normal adults showed no difficulty in grasping the problem of Box V (three interconnected levers), but only thirteen out of 200 mental defectives approached this level. It is clear, therefore, that the grasping

of spatio-mechanical relationships of this complexity presents serious difficulty to the mentally defective.

(4) Except for the thirteen giving truly intelligent responses the time factor does not seem to be important, but a short time period necessarily involves the use of a Type I response, and it would seem that a time longer than 20 sec. is incompatible with a normal mentality.

(5) Success with this test although roughly related to general intelligence shows no close correspondence. For example, among the thirteen who gave the superior response, one showed an Intelligence Quotient of only 44 as tested by the combined Binet-Porteus(7), (8) tests while one with an Intelligence Quotient of 85 appeared in Type II of Class A.

Like other tests this shows the inability of the defective to construct or hold complicated patterns involving spatial relationships, and this must depend on the paucity of the cortical neurones, but the want of exact correlation with general intelligence shows at the same time that this function is not uniformly distributed, and that while a '*g*' factor plays a part an '*S*' factor must also be taken into account. It would seem that the appreciation of spatial relationships and manipulation of spatial configurations is an important study, for it is from these spatial configurations and their demarcations, analyses and integrations, that speech, the basis of all higher mental processes, is derived.

Further, the study of such configurations is closely bound up with the study of images in general, and to these we may next direct our attention.

V. BLOT TESTS.

(1) *Preliminary observations.*

The previous experiments suggest the importance of image formation and the following experiments were undertaken to investigate imagery in mental defectives.

Rorschach in his book *Psychodiagnostik* suggested the use of blots to investigate the visual imagery of individuals. The idea was to obtain as nearly as possible a meaningless pattern and to discover what the subject read into it. This idea is of course not new, for it has long been recognized in aesthetics that any pattern may be used as a stimulus to imagery. Blake instructed his pupils to study the damp stains on plaster walls, while the old pastime of seeing pictures in the fire depends on the same principle.

(2) *Method of experiment.*

In our experiments, which were carried out by Miss J. Worger assisted by Miss M. Havers, in the lower grade group of mental defectives, we used the blot test on the same two classes of 100 relatively high grade girls and 100 boys of the imbecile and idiot class. Our objects were (1) to test the visual imagery aroused by the presentation of the blots, (2) the degree to which the colour was noticed and mentioned, (3) the degree to which any attempt was made to describe and define the image, apart from giving a definite 'configuration,' *e.g.* tree, butterfly, etc. It is interesting to note that out of 711 responses obtained from 600 presentations, *i.e.* three 'blots' to each of 200 subjects, only two responses of 'ink' were obtained and one of 'blot.' Our material consisted of three

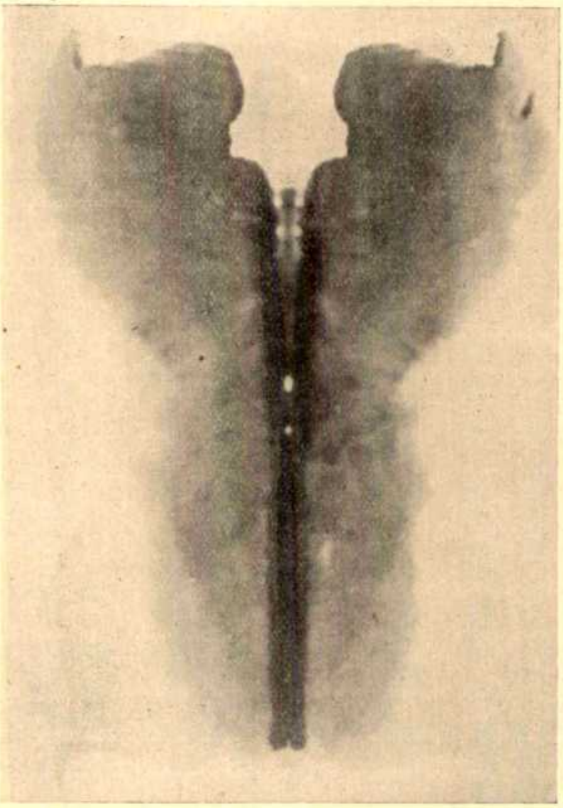

Fig. 10.

blots (Figs. 10, 11, 12), which were presented to each subject in a definite order of complexity. These were prepared by shaking a few drops of blue-black and red ink on a sheet of ordinary paper and folding it over symmetrically while the ink was wet. In this way coloured patterns were obtained of more or less suggestive form.

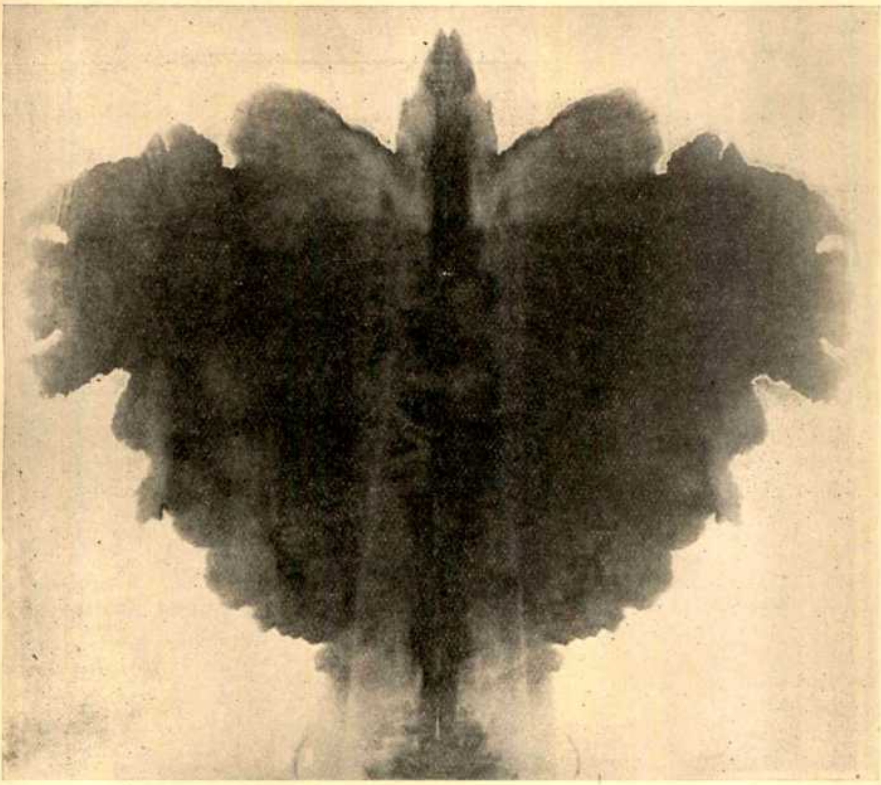

Fig. 11.

We classified the results as to imagery into three groups:

(1) Good; when many images were given as a response or when the response was such as to denote a vivid imagination with power to combine images, as for example, in response to the third blot: "A woman standing by a tree with a dog."

(2) Poor; when the subject failed to give at least three responses to the series of three blots.

(3) Fair; all results between the 1st and 2nd groups.

(3) *Results.*

(i) *Goodness of imagery.*

In the 100 high grade girls, we obtained responses from all and found that:

18 showed good imagery.
59 fair imagery.
23 poor imagery.

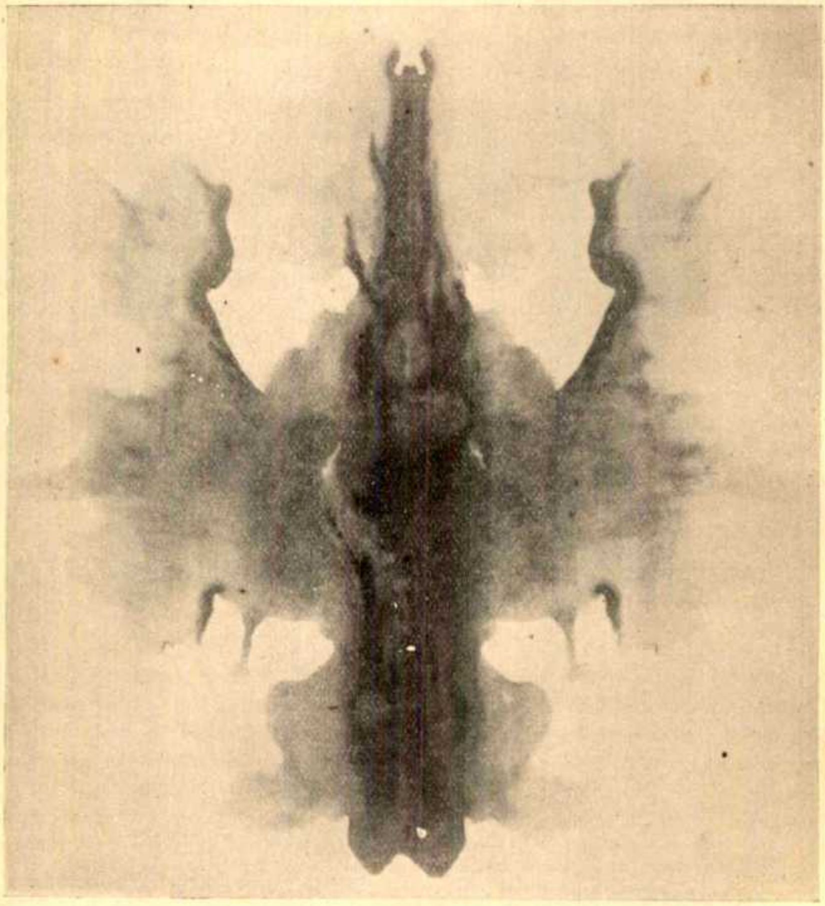

Fig. 12.

The 100 low-grade boys were grouped into 36 idiots and low-grade imbeciles who could not be induced to give any response at all, and 64 imbeciles rising towards the lower levels of feeble-mindedness, all of whom gave some sort of response.

Of these 64 high-grade imbeciles, 2, or 3 per cent., showed good imagery,
22, or 34 per cent., showed fair imagery,
and 40, or 63 per cent., showed poor imagery.
Of the total number of both classes, 20, or 10 per cent., showed good imagery,
81, or 40·5 per cent., showed fair imagery,
and 63, or 31·5 per cent., showed poor imagery,
while 36, or 18 per cent., gave no response.

It will be seen, therefore, that richness of imagery corresponds roughly but by no means exactly with intellectual capacity.

It was found that 40 of the 100 high-grade girls and 5 of the higher grade imbeciles, *i.e.* 22·5 per cent. of the total, showed descriptive tendencies in this test. By descriptive we mean that the subject would enumerate parts of the image evoked, *e.g.* if she said a blot was a butterfly, she would go on to say there are its wings, that is its back and there are its legs.

In the high-grade cases 6 out of 18 'good imagery' subjects were descriptive. Adding to this total 2 'good' cases from the lower group who were not descriptive the percentage is 30 per cent.

In the high-grade cases 24 out of 59 fair imagery subjects, *i.e.* 41 per cent. were descriptive. Adding 2 out of 22 fair in the low-grade group we have 26 out of 81, or 32 per cent.

In the high-grade cases 10 out of 23 poor imagery subjects, *i.e.* 44 per cent. were descriptive. Adding 3 out of 40 poor imagery subjects in the low-grade group we have 13 out of 63, or 21 per cent.

It would appear that the tendency to descriptiveness occurs in those of higher intellectual capacity, but has little relation to the richness of imagery except in so far as this latter has relation to intellectual capacity.

Observation of colour was found to occur in 49 of the higher grade girls, in 6 higher grade imbeciles and 2 lower grade imbeciles. Relating this to types of imagery we found that in the higher grade girls 8 out of 18 good imagery subjects, *i.e.* 44 per cent., noticed colour. Adding 2 good imagery subjects from the lower group who did not notice colour the percentage is 40.

28 out of 59 fair imagery subjects, *i.e.* 47 per cent., in the higher group noticed colour. Adding 2 out of the 22 similar subjects in the lower group we find the percentage is 37.

13 out of 23 poor imagery subjects, *i.e.* 57 per cent., noticed colour in the higher group. If we add 6 out of 40 in the lower group the percentage is 30.

It would appear therefore, that the noticing of colour has little to do with the richness of imagery, but that those of higher intellectual capacity have a much greater tendency to notice and mention it than those of lower mentality.

(ii) *Varieties of images.*

The sort of images formed is an interesting study. It was found that 144 different images were described in the 711 responses obtained from the 600 tests involved by showing three blots to each of 200 subjects. The images fall roughly into the following groups:

Animals	27
Things that fly (birds, bats, etc.) ...	19
Human beings or parts of the body ...	15
Insects (butterflies, beetles, etc.)	13
Miscellaneous	41

For Blot 1 there were 259 responses (61 varieties of response) from 200 subjects, or, excluding those who gave no sort of response to any of the three tests, 164 subjects. Amongst these responses, 'butterfly' was prime favourite, occurring 60 times (24 per cent.). 'Dog' came next with 25 responses, and 'cat' and 'bear' with 12 each. In spite of the preponderance of 'butterflies,' common animals were the favourite response occurring 94 times. Thus:

Dog	25	Sheep	7	Bull	1
Cat	12	Lion	6	Elephant	1
Bear	12	Rabbit	6	Donkey	1
Animal	11	Animal skin	2	Fox	1
Horse	7	Lamb	2		
				Total	94

'Insects,' using this as a general term, came next occurring 88 times as follows:

Butterfly	60	Fly	4	Moth	1
Bee	7	Insect	2	Meatfly	1
Dragonfly	5	Beetle	2	Horse stinger	1
Wasp	5				
				Total	88

Next came 'flowers and trees' 21, thus:

Flower	10	Iris	1
Tree	7	Buttercup	1
Violet	1	Fruit	1
		Total	21

and 'flying things' also 21, thus:

Bird	7	Parrot	2
Bat	6	Owl	1
Dragon	4	Wings	1
		Total	21

The next group comprised parts of the human body or persons, including one imbecile who thought the blot indicated the examiner herself. These totalled 17, thus:

Man	4	Lady	2	Nose	1
Body	3	Tongue	1	Mouth	1
What was meant for buttocks but not indicated so crudely	2	Head	1	Examiner	1
		Fingers	1		
				Total	17

Two subjects were found to declare the blot was 'ink,' while one said it was a blot. Other responses were as follows:

Cloud	2	Wall	1	Seed cake	1
Vase	2	Gollywog	1	Picture	1
Fan	1	Doll	1	House	1
Mat beater	1	Mountain	1	Ornament	1
				Total	14

In the case of the second blot, as might be expected with a more complicated but more difficult pattern, there is an increase of variety of response, viz. 72, while the actual number of responses is less, viz. 248. In this group 'tree' is prime favourite with 54 responses, 'butterfly' falling to second place with 21, 'flower' coming next with 20. The vegetable group comes an easy first with 107, thus:

Tree	54	Bush	2	Rose	1
Flower	20	Fir tree	2	Flower pot	1
Leaf	6	Cauliflower	1	Basket of flowers	1
Cabbage	6	Brussels sprout	1	Sycamore tree	1
Vase of flowers	4	Chrysanthemum	1	Ash tree	1
Plant	3	Fern	1	Palm tree	1
				Total	107

'Flying things' come next with 51, thus:

Bird	18	Pigeon	2	Wings	1
Bat	16	Peacock	1	Starling	1
Eagle	6	Stork	1	Woodpecker	1
Blackbird	2	Aeroplane	1	Crow	1
				Total	51

The insect group number 31, thus:

Butterfly	21	Wasp	2
Dragonfly	4	Beetle	1
Moth	3		
		Total	31

Animals are various but few, comprising 16 responses, thus:

Mouse	2	Animal skin	1	Cow	1
Bull	2	Dog	1	Buffalo	1
Pony	1	Crocodile	1	Rat	1
Cat	1	Animal	1	Fish	1
Bear	1	Rabbit	1		
				Total	16

Clouds, etc., come next with 12 responses, thus:

Cloud	9	Smoke	1
Stormy sky	1	Fog	1
		Total	12

Then 'landscape phenomena' with 10 responses, thus:

Rock	4	Pool of water	1
Mountain	4	Fountain	1
		Total	10

In the miscellaneous group we have 19 including, according to an imbecile, a portrait of himself:

Map	5	Filleted plaice	1	Chinese puzzle	1
Lighthouse	2	Beehive	1	Parcel	1
Vase	2	Flag	1	Me, *i.e.* subject himself	1
Star fish	1	Church spire	1		
Basket	1	Burning incense	1		
				Total	19

For the third blot, the most complex of the series, we again find a decrease in responses, viz. 204, with an increase in variety, viz. 84. 'Tree' is again the favourite response, but only occurs 23 times, 'flower' being second with 18 and 'frog' third with 12 responses. The largest group was the animal group which showed 63 responses, thus:

Frog	12	Crocodile	3	Gorilla	1
Bear	7	Sheep	3	Goat	1
Animal	6	Cat	3	Cow	1
Dog	5	Elephant	2	Deer	1
Lion	4	Fox	2	Camel	1
Horse	4	Lion skin	1	Tiger	1
Fish	4	Animal skin	1		
				Total	63

Next came the vegetable group with 56, thus:

Tree	23	Logs of wood	1	Blossom	1
Flower	18	Petals	1	Plant	1
Flowerpot	3	Ivy leaves	1	Branch	1
Bush	2	Oak leaf	1	Beech nut	1
Fir tree	1	Leaf	1		
				Total	56

Then the human body or parts of it with 28 responses, thus:

Skeleton	7	Person dancing	2	Head	1
Man	5	Body	1	Nose	1
Face	4	Clown	1	Legs	1
Statue	3	Lady	1	Fingers	1
				Total	28

Next the insect group with 16, thus:

Dragonfly	4	Spider	2	Wasp	1
Fly	3	Crab	1	Bee	1
Butterfly	2	Beetle	1	Mosquito	1
				Total	16

Then the 'flying things' group with 15, thus:

Bat	7	Eagle	1	Ostrich	1
Bird	2	Swallow	1	Dead chicken	1
Owl	1	Duck	1		
				Total	15

The miscellaneous group were as follows:

Cloud	3	Map	1	Scarecrow	1
Motor	2	Coat of many colours	1	Water	1
Burning Guy Fawkes	1	Candlestick	1	Sea	1
Paper lantern	1	Design	1	House	1
Rag doll	1	Puzzle	1	Mountain	1
Rockery	1	Queen of Hearts	1	Seed cake	1
				Total	21

While two seemed to see a composite picture, thus:

A woman by a tree with a dog,
A mountain with someone standing on it.

Taking all three blots together, we find that the responses which occur more than ten times in all are as follows out of a total of 711 responses:

Tree	84 times	Bird	27 times	Dragonfly	13 times
Butterfly	83 times	Bear	20 times	Frog	12 times
Flower	48 times	Animal	18 times	Horse	11 times
Dog	31 times	Cat	16 times	Sheep	10 times
Bat	29 times	Cloud	14 times	Lion	10 times

The general responses are therefore quite simple and definite 'configurations' suggested from the somewhat nondescript patterns portrayed, and these can be produced by individuals quite low in the intellectual scale, having a 'mental age' of certainly not more than 4 to 5 years.

(4) *Conclusions.*

(1) The paucity of imagery for the whole group is very noticeable and this is related fairly closely to intellectual capacity; the higher the intelligence on the whole the richer the imagery, but rich imagery does occur in the lower grades.

(2) The tendency to description seems to go with higher intelligence

but does not correspond to richness of imagery. This would seem to be indicative of a power to analyse perceptual configurations.

(3) The noticing of colour is most frequent in the higher grades but does not occur in the lower grade subjects, and probably represents an increased richness in the configurations involving some degree of '*s*' factor.

(4) There is practically no abstract imagery, the images corresponding to the limited environmental stimuli to which these cases are restricted owing to their poorly developed nervous systems. This corresponds to the difficulty in manipulating any sort of unusual or complicated configuration in the spatial field.

(5) There is very little combination of ideas into any sort of composite picture, the images which are perceived being simple configurations. This seems to explain why unfamiliar spatial configurations are more difficult than simple word configurations; also, that directly spatial configurations become complex their analysis and consequently their manipulation becomes difficult or impossible.

The mental defective, therefore, gives us an opportunity of studying the capacity for dealing with the simpler phenomenal perceptual configurations, and further experiments might throw light on how these develop from the anthropoid to the full human level.

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TESTS OF PERSEVERATION.

II. THEIR RELATION TO CERTAIN PSYCHOPATHIC CONDITIONS AND TO INTROVERSION[1].

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I. *Introductory* (pp. 114–5).
II. *Perseveration and certain psychopathic conditions* (pp. 115–6).
III. *Perseveration and introversion* (pp. 116–126).
IV. *Summary* (p. 26).
References (p. 126).

I. INTRODUCTORY.

FROM the first use of the term 'perseveration' up to the time of the present investigation, introversion (or one of its various synonyms) has almost invariably been regarded as associated with strong perseveration, and extraversion as associated with weak perseveration. Correlations have also been frequently claimed between perseveration and certain psychopathic states, melancholia especially being associated with strong perseveration and mania with weak perseveration. Thus has been built up a whole romantic literature round this theme, without, however, any adequate experimental proof being offered of its foundation. Indeed with the exception of the Dutch school represented by Heymans and Wiersma (1), little or no experimental work has been done either to prove or to disprove what was, after all, mere assumption. In 1906 these two psychologists, using the colour wheel, and light-adaptation and tactile tests applied to mental patients and normal people, claimed to find that melancholics were stronger and maniacs weaker perseverators than normal people. But their data were far from satisfactory and it was therefore decided to re-test their theory in the present investigation, and at the same time to examine the general relation between perseveration and introversion.

The tests employed to measure perseveration in both these experiments were the 'S Test' (S.), the 'Triangle Test' (△), the 'Alphabet and Number Test' (A.N.) and the 'Mirror Image Test' (M.I.) (2). Briefly,

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[1] This research was carried out with the help of a grant to the Institute from the Rockefeller Foundation.

they consist in writing letters and figures in the usual manner for 1 min. (*e.g.* S S S S S S ...), reversing the design for another minute (*e.g.* Ƨ Ƨ Ƨ Ƨ Ƨ Ƨ ...), and finally alternating the first with the second for 2 min. (*e.g.* S Ƨ S Ƨ S Ƨ ...). Usually these tests are applied five times after five practices, and the mark for perseveration is reckoned from the last five applications by subtracting the average correct total of the second 2 min. work (y) from the average total of the first 2 min. work (x).

II. PERSEVERATION AND CERTAIN PSYCHOPATHIC CONDITIONS.

In order to determine the relation between various psychopathic states and perseveration 144 patients at the Maudsley Hospital were tested for perseveration, and the medical staff supplied the information as regards their mental and nervous symptoms by writing a cross opposite the name and under the psychopathic symptom which was considered specially marked. The average perseveration mark for all the 144 subjects was determined, and then the averages of those marks which fell (*a*) above, and (*b*) below that mark were calculated respectively. By the three dividing lines thus obtained the subjects were segregated into four groups according to the degree of their perseveration. The strongest perseverators constituted the group with the highest marks and were called extreme perseverators (E.P.); they were followed by the moderate perseverators (M.P.), the moderate non-perseverators (M.N.) and finally by the extreme non-perseverators (E.N.), who were the group with the lowest marks for perseveration. The crosses indicating the mental and nervous symptoms were separately totalled for each of these four groups. When the results were tabulated, the following figures were obtained:

No. in each group	Degree of perseveration	Mania	Melancholia	Obsessions	Hysteria	Anxiety	Paranoidstate	Delusions	Hallucinations	Self-reproach	Antagonism	Erotism	Suspicion	Sensitiveness	Depression	Ideas of reference	Regressive habits	Poor application
35	E.P.	0	20	5	2	17	8	4	5	12	4	2	8	19	20	3	1	12
40	M.P.	1	17	3	5	15	7	12	6	7	4	4	8	9	20	4	2	9
33	M.N.	2	8	1	10	12	4	12	2	10	4	0	3	8	23	3	3	14
36	E.N.	5	4	1	17	12	2	2	3	6	5	1	1	7	28	1	1	11
A.																		
71*	E.P. & E.N.	5	24	6	19	29	10	6	8	18	9	3	9	26	48	4	2	23
73†	M.P. & M.N.	3	25	4	15	27	11	24	8	17	8	4	11	17	43	7	5	23
B.																		
75‡	E.P. & M.P.	1	37	8	7	32	15	16	11	19	8	6	16	28	40	7	3	21
69§	M.N. & E.N.	7	12	2	27	24	6	14	5	16	9	1	4	15	51	4	4	25

* Extremes. † Moderates. ‡ Perseverators. § Non-perseverators.

Applying Yule's formula $e_{12}{}^2 = \frac{p_1 q_1}{n_1} + \frac{p_2 q_2}{n_2}$, where p_1 is the proportion of the cases of the one kind, p_2 is the proportion of the cases of the other kind, n_1 is the number of cases used to get p_1 and n_2 is the number of cases used to get p_2, $q_1 = 1 - p_1$, $q_2 = 1 - p_2$. The following data resulted:

A. Delusions: P.E. = 0·04, $p_1 - p_2$ = 0·3, which is nearly 9 times as large as P.E.
B. Melancholics: P.E. = 0·04, $p_1 - p_2$ = 0·4, which is 10 times as large as P.E.
B. Suspicion: P.E. = 0·03, $p_1 - p_2$ = 0·2, which is nearly 7 times as large as P.E.
B. Hysteria: P.E. = 0·05, $p_1 - p_2$ = 0·3, which is 6 times as large as P.E.
B. Mania: P.E. = 0·02, $p_1 - p_2$ = 0·08, which is 4 times as large as P.E.
B. Paranoid state: P.E. = 0·03, $p_1 - p_2$ = 0·13, which is 4 times as large as P.E.
B. Obsessions: P.E. = 0·03, $p_1 - p_2$ = 0·08, which is nearly 3 times as large as P.E.
B. Erotism: P.E. = 0·02, $p_1 - p_2$ = 0·07, which is more than 3 times as large as P.E.
B. Hallucinations: P.E. = 0·03, $p_1 - p_2$ = 0·08, which is nearly 3 times as large as P.E.

From these data we might conclude that delusion tends to go with the intermediate forms of perseveration; that melancholia, suspicion and the paranoid state tend to go with extreme perseveration; and that hysteria and mania tend to go with extreme non-perseveration. The figures for the other symptoms are certainly not significant. The experimenter does not wish to attach undue importance to the significance of these results. In the first place, the number of cases grouped under the different symptoms was sometimes very small. Secondly, there was some lack of uniformity of opinion among the physicians who supplied the information, as regards the symptoms, which necessarily made the classification difficult and uncertain. But so far as they go, they support Heyman and Wiersma's conclusions, although they are not in exact harmony with Wynn Jones's (3).

III. PERSEVERATION AND INTROVERSION.

A list was compiled of all the characteristics, which at some time or other in the development of the concept of 'introversion' have been associated with it. These characteristics were classified on a circular psychological scheme, in the light of Gross's primary and secondary functions (4), purely schematic perhaps, but nevertheless making some order and arrangement possible. The following is the list:

General scheme.

1. *Accentuated primary function.*

(*a*) Is unable to relax easily. (Is in a state of general tension—has a feeling of being 'strung up.')

(*b*) Has alert attention. (Is wide awake both to environment and to opportunities.)

(*c*) Has vivid impressions. (Is easily and deeply moved by experiences.)

2. *Prolonged secondary function.*

(*a*) Experiences difficulty in doing more than one thing at the same time (such as playing the piano and carrying on a conversation, or listening to music and writing a letter).

(*b*) Is easily confused and embarrassed under varied stimuli (*e.g.* he 'loses his head' in crossing a busy street).

(*c*) Experiences difficulty of apprehension in initial stages of presentation. (Illustration: two people, *A* and *B*, of equal intelligence are taught to drive a car. *A* grasps the mechanism at once; *B* is confused at first and takes some time to learn—although eventually both drive equally well.)

(*d*) Has a puzzled look on face.

(*e*) Experiences the persistent recurrence of a tune to the mind (to the extent that it sometimes becomes a nuisance).

(*f*) Feels the after-effect of a voyage or train journey (*e.g.* the water or other objects still seem to pass by after the ship or the train has stopped).

(*g*) Suffers from ill-effect of sitting with his back to the engine. (Suffers from uncomfortable and giddy feelings sometimes followed by sick headache.)

(*h*) Has difficulty in adapting his eyes to light contrasts (gets very blind when going into cinema and sees flickers on the wall when entering his house on a bright sunny day).

(*i*) Experiences difficulty in putting an idea out of his mind (even though it is unpleasant).

(*j*) Has dreams more commonly about the past than of a premonitory nature.

(*k*) Finds that it takes a long time for his emotions to subside after the cause of the disturbance has been withdrawn.

(*l*) Experiences difficulty in changing from one occupation to another.

(*m*) Experiences difficulty in resuming work after interruption.

(*n*) Finds that his temptations persist and will not be shaken off (*e.g.* he succeeds in passing a public house but then returns to celebrate the victory).

(*o*) Has great difficulty in giving up an unsolved problem.

3. *Undue integration of ideas.*

(*a*) Interested in many things at the same time.

(*b*) Experiences a tendency to regard all external stimuli in relation to prevailing idea—even though they are obviously irrelevant.

(*c*) Has his ideas highly developed and deeply rooted (*i.e.* he knows his subject in all its details, holds strong convictions and is slow in changing them).

(*d*) Experiences inclination to bring what is separate into single view (he tries to convince himself that all the subjects he is interested in, no matter how different in character, dovetail into each other and support each other in such a way as to form an ideal combination).

(*e*) Has a narrow range of thought (is interested in only a few things).

(*f*) Works with great spurts of short-lived energy.

(*g*) Confines his energy to the subjects he is mainly interested in.

4. *Undue isolation of ideas.*

(*a*) Experiences difficulty in working with implements other than his own even though they are identically the same.

(*b*) Is averse to change, for no apparent reason.

(*c*) Is fearful of attacking a new job.

(*d*) Is given to abstract thinking (often occupied with his own thoughts).

(*e*) Has a high feeling-tone associated with his ideas, so that he has little energy left for carrying them into execution.

(*f*) Experiences delight in developing ideas for their own sake, and not principally for the financial gain or the fame that might result.

(*g*) Develops a feeling of attachment to things used to (*e.g.* he looks upon old clothes as if they were his friends).

(*h*) Has difficulty in realising his ideal, because of inability to adapt himself to his environment (*e.g.* by being unpractical or too individualistic and wanting to do everything himself).

(*i*) Derives enjoyment from talking about himself.

(*j*) Is critical of others.

(*k*) Is impatient of criticism.

(*l*) Is obstinate.

(*m*) Is fearful of making enemies.

(*n*) Derives extra pleasure from harmonious surroundings (as contrasted with the person who brightens up with a fight).

(*o*) Has very little social consideration: does not study the comfort of other people.

(*p*) Has pronounced individual personality, being noticed and discussed no matter how much he tries to keep in the background.

(*q*) Is fearful of having his idea tested (it might not work, and the pleasure of anticipating success will disappear).

(*r*) Prefers to work alone.

(*s*) Is subject to day-dreaming.

(*t*) Is reticent.

(*u*) Is competitive.

(*v*) Is a poor loser.

5. *Undue conflict of ideas.*

(*a*) Comes into conflict with his environment, even though he starts well and is determined not to disturb favourable relations.

(*b*) Suffers from tormenting emotions (*e.g.* bitterness, fear, anxiety, etc.).

(*c*) Is given to comparing his own feelings with those of others, and finding that his are different either in degree or in quality.

(*d*) Heaps up libido (which manifests itself during idleness in irritability, restlessness, dissatisfaction, etc.).

(*e*) Suffers from exaggerated introspection. Inner facts and feelings are always in the foreground.

(*f*) Has a tendency to exaggerate the feeling behind his ideas.

(*g*) Lacks harmony (is in conflict with himself).

(*h*) Has a dual personality (not necessarily as marked as that of Dr Jekyll and Mr Hyde).

(*i*) Thinks that people do not understand him.

6. *Resulting temperament.*

(*a*) Feels intensely, but 'consumes his own smoke.'

(*b*) Displays great emotionality: feels strongly and shows it.

(*c*) Has 'ups and downs' in mood for no apparent reason.

(*d*) Is 'touchy.'

(*e*) Suffers from melancholia.

(*f*) Gets homesick (unduly so, and without adequate cause).

(*g*) Takes things too seriously.

(*h*) Seeks consolation in nature rather than in amusements.

(*i*) Views outside world with distrust (*e.g.* he has no faith in human nature).

(*j*) Has mental aberrations (so that when he looks back from one period of his life to another he thinks that he must have been mad to do the things he did).

(*k*) Shows violent reaction to punishment: instead of clearing the atmosphere it brings about a feeling of hatred or revenge.

(*l*) Is 'nervous.'

(*m*) Is silent and anxious (both symptoms must be present).

(*n*) Lacks tact.

(*o*) Leads solitary existence: keeps in background on social occasions.

(*p*) Experiences difficulty in restoring relations on right lines once they have been disturbed.

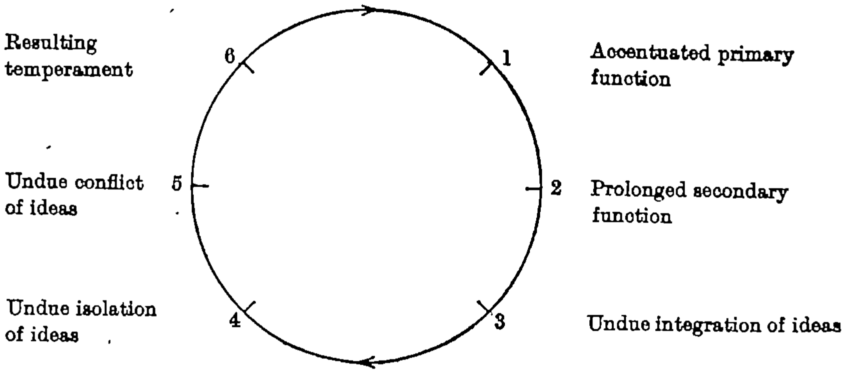
(*q*) Suffers from a feeling of inferiority.

(*r*) Experiences a feeling of irritation against environment for no apparent reason.

(*s*) Suspicious of motives of others.

(*t*) Pays serious attention to rumours.

(*u*) Is effeminate. (The man prefers a quiet domestic life to risking life and limb in big-game hunting or flying the Atlantic: the woman is perfectly satisfied with home and children, and finds no pleasure in mountaineering, playing athletic games, or going in for politics[1].)


Resulting temperament
6
1
Accentuated primary function
Undue conflict of ideas
5
2
Prolonged secondary function
Undue isolation of ideas
4
3
Undue integration of ideas

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[1] The medical staff judged patients under this head by their sexual life. It was found that many extreme perseverators (men) had not succeeded in consummating marriage, and that the extreme non-perseverators (women) were generally masculine in build and hair grew on their faces.

A list of these qualities was supplied to each of the members of the medical staff at the Maudsley Hospital, and the work was started on those patients in the hospital who had recovered so far that they were on the point of being discharged. All of them had been subjected to some previous psychological analysis, and could therefore perhaps be relied upon to give a fairly accurate account of themselves. This part of the work lasted over a period of 18 months, during which 236 subjects were tested for perseveration and intelligence and estimated for introversion.

The subjects were taken individually and in small groups by the members of the staff, six in all, who were in constant consultation with

	E.P.	M.P.	M.N.	E.N.
1 (*a*)	57	43	65	67
(*b*)	85	85	58	40
(*c*)	57	71	75	63
2 (*a*)	85	41	42	68
(*b*)	43	47	58	48
(*c*)	59	35	28	73
(*d*)	59	33	38	73
(*e*)	45	63	75	83
(*f*)	37	55	33	13
(*g*)	35	39	12	12
(*h*)	33	55	38	68
(*i*)	88	57	62	80
(*j*)	77	27	28	60
(*k*)	45	59	65	40
(*l*)	33	41	28	77
(*m*)	27	59	52	63
(*n*)	31	51	43	70
(*o*)	95	29	37	55
3 (*a*)	37	41	48	53
(*b*)	31	67	67	42
(*c*)	45	79	78	30
(*d*)	31	55	68	43
(*e*)	17	75	65	42
(*f*)	57	49	73	80
(*g*)	45	51	77	83
4 (*a*)	39	55	68	58
(*b*)	51	33	53	68
(*c*)	64	55	42	78
(*d*)	35	65	70	58
(*e*)	67	63	72	58
(*f*)	61	59	42	42
(*g*)	85	80	75	70
(*h*)	39	27	77	40
(*i*)	27	25	27	67
(*j*)	43	41	75	85
(*k*)	51	49	85	63
(*l*)	61	37	37	67
(*m*)	73	81	37	73
(*n*)	87	97	73	70
4 (*o*)	43	13	45	60
(*p*)	11	59	53	35
(*q*)	29	53	67	60
(*r*)	37	69	65	38
(*s*)	71	53	62	62
(*t*)	49	65	87	55
(*u*)	47	39	80	50
(*v*)	11	39	10	35
5 (*a*)	31	39	13	47
(*b*)	63	33	73	67
(*c*)	67	41	35	65
(*d*)	53	63	63	48
(*e*)	55	43	60	47
(*f*)	59	43	32	53
(*g*)	45	41	52	70
(*h*)	23	43	52	75
(*i*)	69	41	27	68
6 (*a*)	87	85	63	60
(*b*)	57	69	62	77
(*c*)	49	47	68	87
(*d*)	63	43	35	52
(*e*)	49	55	28	50
(*f*)	31	55	57	65
(*g*)	93	91	80	55
(*h*)	75	73	42	62
(*i*)	15	21	60	50
(*j*)	45	35	57	52
(*k*)	43	31	43	58
(*l*)	87	67	47	85
(*m*)	51	55	68	85
(*n*)	27	25	43	58
(*o*)	61	59	55	28
(*p*)	27	29	47	43
(*q*)	43	19	38	62
(*r*)	25	31	33	50
(*s*)	13	21	47	48
(*t*)	15	45	47	55
(*u*)	91	73	17	17

Total No. of subjects			
E.P.	M.P.	M.N.	E.N.
54	58	61	63

each other, and who had an intimate knowledge of the patients and their case histories. Each interview, more often than not in the presence of the experimenter, lasted from 2½ to 3 hours, and was spread over a period of several days. The characteristics were taken one by one and were fully explained by the medical officer and experimenter, who also answered all questions bearing on the point. The patient was asked to indicate by writing a cross on the list those characteristics which were specially well-marked in his mental make-up. The subjects were then tested for perseveration, ranked on the average of the four tests applied five times, and were divided into the four equal groups of extreme perseverators (E.P.), moderate perseverators (M.P.), moderate non-perseverators (M.N.), and extreme non-perseverators (E.N.). The characteristics marked for each group were counted, and the figures shown on p. 120 resulted, which for the sake of comparison were brought to percentages of the actual number of crosses present in relation to the maximum possible number for each characteristic.

Applying the formula $e^2 = \frac{p_1q_1}{n_1} + \frac{p_2q_2}{n_2}$, the following characteristics proved definitely significant for the different groups, E.P., M.P., M.N., E.N., in the manner in which they are classified below.

Extreme perseveration.

6 (*g*) Takes things too seriously.
6 (*o*) Leads solitary existence and keeps in background on social occasions.
4 (*g*) Develops a feeling of attachment to things used to.
2 (*m*) Has no difficulty in resuming work after interruption.
4 (*q*) Has no fear of getting idea tested.
4 (*v*) Is not a poor loser.

Moderate perseveration.

4 (*b*) Is not averse to change.
4 (*o*) Is very considerate.
6 (*q*) Has no feeling of inferiority and bitterness.
4 (*h*) Has marked ability to adapt to environment.
5 (*b*) Has no tormenting emotions.

Extreme and moderate perseveration.

1 (*b*) Has alert attention.
2 (*g*) Experiences ill effect of sitting with back to engine.
4 (*f*) Experiences delight in developing ideas for their own sake.
4 (*n*) Harmony in surroundings gives extra pleasure.
6 (*a*) Feels intensely.
6 (*h*) Seeks consolation in nature.
6 (*l*) Is nervous.
6 (*u*) Is effeminate.

Extreme non-perseveration.

2 (*e*) Experiences persistent recurrence of tune, etc. to the mind.
3 (*g*) Conserves energy for things interested in.
3 (*a*) Interested in many things at the same time.
3 (*f*) Works with great spurts of short-lived energy.
5 (*h*) Has dual personality.
6 (*f*) Experiences homesickness.
6 (*m*) Is silent and anxious.
6 (*r*) Feels irritation against environment.
6 (*t*) Pays serious attention to rumours.
2 (*l*) Experiences difficulty in changing from one occupation to another.
4 (*i*) Derives enjoyment from talking about himself.
5 (*g*) Lacks harmony.
4 (*o*) Has very little social consideration.
6 (*q*) Feels inferior and bitter.
2 (*h*) Has difficulty in adapting eyes to light contrasts.
2 (*n*) Experiences persistence of temptation.
2 (*f*) Experiences no after-effect of voyage or train journey.

Moderate non-perseveration.

4 (*u*) Is competitive.
4 (*m*) Is not afraid of making enemies.
5 (*a*) Does not come in conflict with environment.
6 (*e*) Does not suffer from melancholia.
4 (*v*) Is not a poor loser.
6 (*h*) Seeks consolation in company.

Extreme and moderate non-perseveration.

1 (*a*) Is not able to relax easily.
4 (*j*) Is critical of others.
4 (*k*) Is impatient of criticism.
6 (*c*) Has ups and downs in mood.
6 (*i*) Views outside world with distrust.
6 (*n*) Lacks tact and resourcefulness.
6 (*p*) Has difficulty in restoring relations with other people.
6 (*s*) Suspicions of motives of others.

Moderate perseveration and non-perseveration.

1 (*c*) Has vivid impressions.
2 (*k*) Emotions take a long time to subside.
3 (*b*) Experiences tendency to regard all external stimuli in relation to prevailing idea.
3 (*c*) Has ideas highly developed and deeply rooted.
3 (*d*) Is inclined to bring what is separate into single view.
3 (*e*) Has a narrow range of thought.

4 (*d*) Is given to abstract thinking and inventing.
4 (*p*) Has a pronounced individual personality.
4 (*r*) Prefers to work alone.
4 (*t*) Is reticent.
5 (*d*) Heaps up libido.

Extreme perseveration and non-perseveration.

2 (*a*) Has difficulty in doing more than one thing at a time.
2 (*j*) Dreams more commonly about the past.
2 (*o*) Experiences inability to give up unsolved problem.
6 (*d*) Is touchy.
2 (*i*) Experiences difficulty in putting an idea out of his mind.
4 (*l*) Is obstinate.
5 (*c*) Given to comparing own feelings with those of others.
5 (*f*) Has tendency to exaggerate feelings behind ideas.
5 (*i*) Thinks that people do not understand him.
2 (*c*) Has difficulty in apprehending in initial stages of presentation.
2 (*d*) Has puzzled look on face.
4 (*c*) Is fearful of attacking new job.

A first glance at the figures and more especially at the rated characteristics brings about a state of confusion of mind that is very disturbing. One has first and foremost to contend with the multiplicity of results in a relatively unknown field, and the task of assimilating them is made all the more difficult because the manner in which they group is both unexpected and contrary to all preconceived ideas and theories. On further examination, however, different explanations suggest themselves and several lines of investigation open up.

One thing seems to be beyond dispute, namely, that the facts before us definitely explode the theory held so long and by such eminent psychologists, that introversion is synonymous with extreme perseveration, or that there is any significant correlation between perseveration and what was generally supposed to be the mental make-up of the introvert. If anything the very opposite is the case. Twenty-four of the characteristics attributed to introversion occur in the tables, specially among the extreme non-perseverators, and only fourteen among the extreme perseverators. Eleven are common to both extremes, four are markedly absent among the extreme perseverators, and only one among the extreme non-perseverators. Moreover a linear correlation between the number of introvert characteristics marked for each subject and the corresponding scores for perseveration proved to be $-0{\cdot}23$, which with a probable error of $\pm 0{\cdot}043$ is definitely significant but negative.

We can go a step further and say that our new classification of these characteristics forces us to alter our time-honoured conception of the introvert, and that it lays down a tentative basis for a new and a more scientific orientation of types. It becomes clear on the most superficial examination that the extreme perseverator tends to be a very different person from the extreme non-perseverator, notwithstanding the fact that they have no less than eleven characteristics in common, and that the intermediate perseverators, as a class, are distinct from and in some respects the very opposite to the extremes, although the moderate perseverator and the moderate non-perseverator differ slightly *inter se*. The seventy-seven rateable qualities which have hitherto been claimed by various psychologists to be characteristic of the introvert have thus, in so far as the figures have proved significant, been broken up into distinct categories.

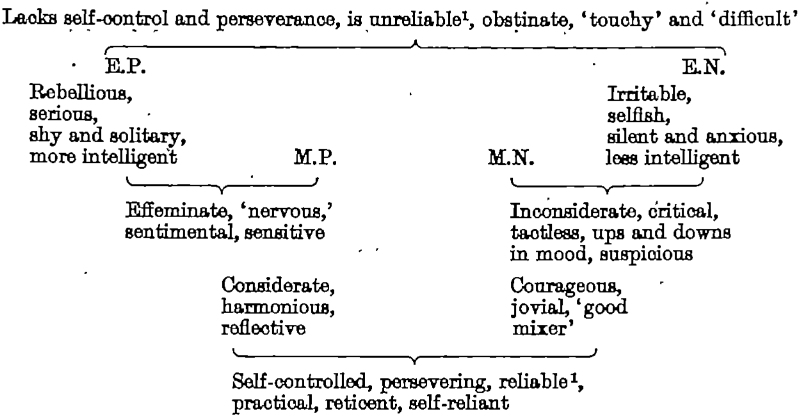
Prima facie it might make for clarity and a more scientific classification if we grouped the characteristics hitherto attributed to the introvert under different degrees of perseveration, which in itself is a native quality of the nervous system, innately different with different individuals, and constituting a general factor of the mind. For the time being, therefore, taking into consideration the results embodied in the experimenter's previous paper (2), we seem justified in classifying persons on the basis of their degree of perseveration, and the outstanding facts are these.

The extreme perseverators and non-perseverators have in common lack of self-control and perseverance to a marked degree, and they are unreliable, obstinate, 'touchy' and difficult. They differ from each other in that the extreme perseverator tends to be effeminate, 'nervous,' sensitive, sentimental, serious, shy, and solitary, whereas the extreme non-perseverator tends to be suspicious, inconsiderate, critical, impatient of criticism, tactless, silent and anxious, and in a state of general tension with ups and downs in mood.

The intermediate perseverators differ from the extreme perseverators in that they are reliable, self-controlled, persevering, easy to get on with, thorough, practical, reticent, and self-reliant. They differ from each other particularly in that the moderate perseverator is considerate, harmonious, and reflective, whereas the moderate non-perseverator is courageous, jovial, and a good mixer.

The average scores made by the extreme perseverators and the moderate perseverators in test No. 33 of the National Institute of Industrial Psychology are 127·7 and 113·1 respectively, whereas the scores made by moderate non-perseverators and extreme non-perseverators are 110·2 and 99·2 respectively.

The following diagram may prove useful in summing up the most outstanding and illuminating results.



Because of the very nature of this work as regards both the subject of research and the wide field it covers, the above results cannot be regarded as more than tentative. So much, however, is certain that within certain limits they afford the basis for a more scientific classification of character types than has hitherto been possible, and that they definitely show the psychologist how, in a practical form, he may help to place the measurement of character at the disposal of the industrial and educational world. For a long time it has been amply evident, both to the industrialist and to the teacher, that intelligence and special ability do not go far without steadfastness of purpose and stability of character; and hitherto these two have been unknown qualities. Now at last, with perseveration as a touchstone, the science of vocational guidance and selection can at least foresee the possibility of dividing, with some measure of success, the stable subjects from the unstable and the reliable from the unreliable, in addition to measuring them for general intelligence and for special abilities.

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[1] See this *Journal* (2), where it was shown that lack of self-control, perseverance and reliability go with the extreme, and the 'persistent' qualities of character with the moderate, forms of perseveration.

IV. Summary.

The supposed association between the melancholic state and extreme perseveration and between the maniacal state and extreme non-perseveration appears to be confirmed by this investigation.

But it directly contradicts the association previously claimed between extreme perseveration and the several qualities of character which have from time to time been supposed to indicate introversion. Not only the extreme perseverator but also the extreme non-perseverator tends to lack perseverance and self-control and to be obstinate, unreliable, 'touchy' and 'difficult.' Whereas the perseverator tends to be 'nervous,' sensitive, effeminate and sentimental, the non-perseverator tends to be inconsiderate, tactless and critical. The moderate perseverator tends to be considerate, harmonious and reflective, the moderate non-perseverator to be courageous, jovial and a 'good mixer.'

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THE FORMATION OF A SIMPLE HABIT IN GUINEA-PIGS.

By G. C. GRINDLEY.

(*From the Psychological Laboratory, Cambridge.*)

I. *Introduction* (pp. 127–9).
II. *Description of apparatus* (pp. 129–32).
III. *Experimental procedure* (pp. 132–5).
  (*a*) *Preliminary training* (p. 132).
  (*b*) *Formation of habit* (pp. 132–4).
  (*c*) *Reversal of habit* (pp. 134–5).
  (*d*) *Extinction of habit* (p. 135).
IV. *Results* (pp. 135–42).
  (*a*) *Formation of habit* (p. 135–8).
  (*b*) *Behaviour between trials* (p. 138).
  (*c*) *Changes in behaviour during each day* (pp. 138–40).
  (*d*) *Variability of the results* (p. 140).
  (*e*) *Reversal of habit* (pp. 141–2).
  (*f*) *Extinction of habit* (p. 142).
V. *Discussion* (pp. 142–7).
  (*a*) *Simplicity of experimental conditions* (pp. 142–3).
  (*b*) *The conditioned reflex theory* (pp. 143–5).
  (*c*) *The law of effect* (pp. 145–6).
  (*d*) *The form of the learning curve* (pp. 146–7).
VI. *Summary* (p. 147).
References (p. 147).

I. INTRODUCTION.

It is often argued that all learning, whether animal or human, consists of the formation of conditioned reflexes. Pavlov, indeed, says that "it is obvious that the different kinds of habits based on training, education and discipline of any sort are nothing but a long chain of conditioned reflexes" (Pavlov, 1927, p. 395), while Hunter suggests that one of the conclusions to be drawn from a careful review of experimental studies of learning is that "all learning is fundamentally of the conditioned reflex type" (Hunter, 1929, p. 622).

One of the most obvious objections to this theory arises from the nature of "trial-and-error" learning, such as in the formation of maze and problem-box habits. It appears, superficially at least, as though the selection of successful responses and the rejection of unsuccessful responses implies that in some way the consequences of an action determine whether the action is to be repeated or dropped out in subsequent trials; and this kind of selection and rejection of responses seems to be different from what is usually meant by 'conditioning.' The nature of trial-and-error learning suggests, in fact, that some kind of retroactive principle, such as that of Thorndike's "Law of Effect" (Thorndike, 1911), must be added to the principles of 'conditioning' in order to explain the phenomena of learning; and this conclusion is suggested particularly strongly by such experiments as those of Blodgett (1929) and Bruce (1930) on the relation of reward to maze performance.

A believer in the conditioned reflex theory is likely to reply to objections of this kind by referring to, among other things, the complexity of the conditions of the experiments from which the "Law of Effect" has been deduced. It may be pointed out that in a maze or problem-box experiment the animal is reacting with a very complex series of muscular movements to a very complex series of sensory stimuli; and that in successive trials neither the stimuli nor the responses are in any strict sense the same. Since, also, no complete record of the stimuli or of the responses is ever kept in these experiments, and since, even if it were, the record would be so complex as to defy analysis, it may be urged that it is useless to attempt to argue on the basis of maze and problem-box experiments about the laws governing the formation of connections between stimuli and responses. In order to find those laws, it may be argued, it is necessary to experiment (as in Pavlov's laboratory) under conditions in which the stimuli to which the animal reacts are simple and controllable, and in which the response which is studied is simple and measurable.

The experiments described in this paper were made in an attempt to study trial-and-error learning when the relevant stimuli and responses were made as simple, and as nearly repetitive in successive trials, as possible. Guinea-pigs were used as experimental animals, and, as in Pavlov's work, the animals were held in a stand in a sound-proof room during the experiments. Each guinea-pig was taught, by means of a reward of carrot which was given immediately after it had made the required response, to turn its head to one side in response to the sound of a buzzer. A discussion of whether the behaviour which was observed in these experiments can be explained on the conditioned reflex theory is given (in Section V (*b*)) after a description of the experiments.

II. DESCRIPTION OF APPARATUS.

Fig. 1 is a diagram of the apparatus. The guinea-pig stood on a wooden platform *PP*, and its body was held relatively still by means of a jacket *J*, and by a number of adjustable pads, of which *B* is one. (The jacket *J*, together with four tapes which were attached to it and which passed round the animal's legs, was fastened to the wooden arm *A*).

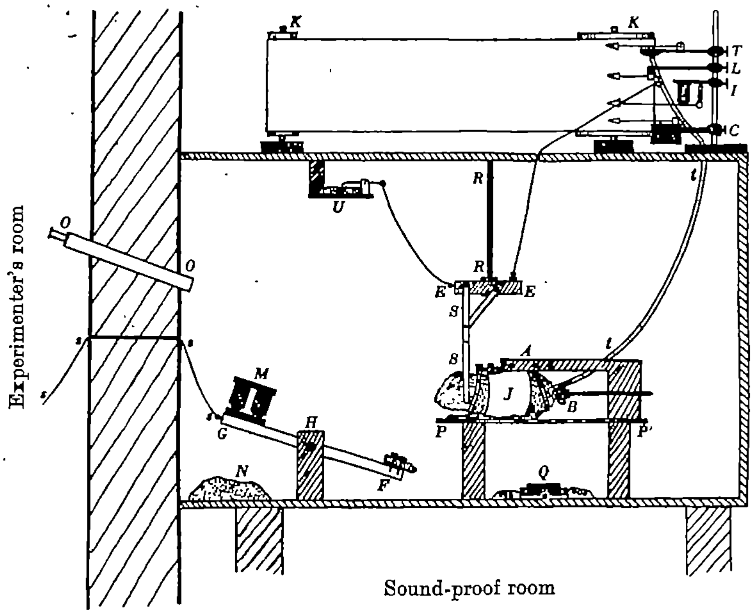

Fig. 1.

The guinea-pig's head was free to move, but two cloth-covered metal strips (of which *SS* is one) pressing lightly against the animal's cheeks ensured that any movement of the head to either side was transmitted to a light wooden arm *EE*. The arm *EE* was mounted on ball bearings so that it could rotate about a vertical axis *RR*, and it was connected by a piece of thin fish line to a lever *L*, working on a kymograph *KK*; so that a kymograph record of sideway movements of the guinea-pig's head was obtained. Vertical movements of the head were not recorded.

In front of the platform *PP* was a lever *FG*, on top of which at the end *F* was fastened a piece of freshly cut carrot, while on the top of the end *G* was a piece of soft iron. The lever could rotate about a horizontal axis *H*, and the experimenter could lift the end *G* (by pulling the string *ssss*) until the iron touched the electromagnet *M*, the attraction of which was sufficient to hold the lever in that position, *i.e.* with the carrot out of reach of the guinea-pig. When the current through the electromagnet was broken the end *G* of the lever *FG* would fall, and the carrot would then rise to a position where it was within easy reach of the guinea-pig. In order to reduce the noise of operation of the lever a pad *N* of soft material was placed below the end *G*, and the piece of soft iron was covered with cardboard.

A kymograph record of the guinea-pig's respiration was obtained by means of a tambour *T* connected through a rubber tube *tt* to a rubber balloon (containing pieces of sponge) which was fastened to the under side of the animal's jacket *J* in such a way that each inspiration produced compression of the balloon. This arrangement recorded the rate and regularity of the animal's respiration, but it did not give a reliable measure of the depth of breathing.

The kymograph had a strip of smoked paper about 8 ft. long, which moved at about 2 in. per min., so that it was possible to obtain a continuous record of the guinea-pig's head-movements and respiration for about 45 min. A chronometer *C*, marking seconds, and an electromagnetic marker *I*, operated by the experimenter, also recorded on the kymograph. The kymograph was worked by clockwork and ran very quietly, as did the lever *L*, the tambour *T*, and the chronometer *C*; but the marker *I* made an appreciable sound when it was operated.

The whole apparatus was in a sound-proof room, since the results were found to be very irregular in a room which was not sound-proof, and the experimenter was in an adjacent room. The experimenter could observe the guinea-pig through a telescope *OO* which passed through the wall. A human subject in the sound-proof room could not hear sounds made by the experimenter in the next room; and throughout the experiments the guinea-pigs gave no sign that they heard any noises from outside.

In addition to observing the guinea-pig, operating the marker *I* and pulling the string *ssss* (to remove the carrot from the animal), the experimenter could switch on or off a buzzer *Q* which was placed beneath the platform *PP* and directly under the guinea-pig, and he could also control the current through the electromagnet *M*. By means of a two-way switch the experimenter could arrange the circuit of the electromagnet either so that the circuit could only be broken by the experimenter (by means of another switch) or so that it was broken automatically when the guinea-pig turned its head to one side. In the latter case the current to the electromagnet passed through a light mercury switch *U* which was connected to the wooden arm *EE* by a piece of fish line in such a way that when the arm was turned to a certain position on the appropriate side the line became taut and opened the switch. The line could be attached to the arm *EE* either in such a way that the switch would be opened by a movement of the animal's head to the right, or so that it would be opened by a movement to the left. The length of the line was such that in either case the switch was opened when the guinea-pig turned its head through about 30° from the normal (directly forward) position. The circuit through the electromagnet could thus be arranged either so that the carrot could be presented to the guinea-pig at will by the experimenter, or so that the carrot was presented immediately the guinea-pig turned its head to an angle of about 30° to one side.

The method of recording head-movements which has been described above is not very satisfactory. Its main defect is that the guinea-pig sometimes thrusts its head forward through the strips *SS*, and sometimes draws its head back, so that equal movements of the arm *EE* do not always correspond to equal rotations of the head. Another defect is that guinea-pigs sometimes struggle to escape from the strips, and that it is difficult to arrange the strips far enough back to make it impossible for the animal to escape from them and yet far enough forward to give a reliable record of the animal's head-movements. Several other methods of automatic recording of head-movements were tried, but, for various reasons, none of them was satisfactory.

In the experiments with some of the guinea-pigs the strips were used; while with others the strips were removed, and the head-movements were merely observed through the telescope by the experimenter and noted down as accurately as possible, there being in front of the platform a rough scale against which the position of the animal's nose could be measured. In the latter case the time of response (*i.e.* the time taken by the guinea-pig to turn its head to a given position after the buzzer had been switched on) was measured by means of a stop-watch, and, immediately the response was made the experimenter released the carrot. The former method (using the strips) gives a much more complete record of the guinea-pig's head-movements during an experiment; but with the latter method the amounts through which the animal must turn in order to obtain the carrot are more nearly constant in successive trials. It is desirable that in future work of this kind the response used should be one which can be recorded automatically and accurately.

Neither of these two methods gave accurate measurements of the time of response when this time was short, since, with either method errors of as much as 0·25 sec. are quite probable. In the learning curves shown in Section IV the times of response are shown as they were measured on the kymograph record, or by the stop-watch; but it must be remembered that, owing to the inaccuracy of the timing, many of the fluctuations in the curves when the time of response is less than about 1 sec. are not reliable.

III. EXPERIMENTAL PROCEDURE.

(a) *Preliminary training.*

Before beginning the actual experiments it was necessary to allow the guinea-pigs to become accustomed to being held in the apparatus, and to eating the carrot when it was presented to them in the way described. A guinea-pig which had had no food for about 20 hours was therefore placed in the apparatus and left there for about 5 min.; after which time the experimenter released the carrot and waited for the guinea-pig to eat. When the animal had taken a few bites the carrot was removed. After an interval of from 1 to 5 min. the carrot was again presented, and this procedure was continued for about half an hour each day.

On the first few days the guinea-pigs usually struggled violently when being put into the jacket, and when they were in the apparatus they often spent much of the time either in struggling or in standing still and rigid, with their eyes wide open, in an attitude which may have been one of fear or anger. Most of the animals would not eat at all in the apparatus on the first day. As the training proceeded the guinea-pigs struggled less when they were being put into the jacket and when they were in the apparatus, and their general attitude suggested placidity rather than fear or anger. Exploratory head-movements, accompanied by sniffing, became more common, and the animals often nibbled the front of the platform on which they were standing. When the carrot was presented they ate after an interval which became progressively shorter as the training proceeded.

The training was continued until, on two successive days, the guinea-pig never hesitated more than about 1 sec. before eating. This usually occurred after from 8 to 15 days of training, and it was found that by this time most of the animals did not struggle at all when being placed in the apparatus. A few animals which, after training periods of about this length, still showed many long hesitations before eating, were discarded.

(b) *Formation of habit.*

When the preliminary training was complete, the procedure on each day was as follows:

The guinea-pig (which had been without food for 20 hours) was placed in the apparatus, the kymograph was started, and the experimenter then immediately left the sound-proof room. After about 4 or 5 min., at a moment when the guinea-pig was facing directly forwards, the experimenter switched on the buzzer and at the same moment set the two-way key in the circuit of the electromagnet so that a movement of the animal's head would cause the carrot to be released. When the guinea-pig made the required movement it, of course, obtained the carrot; but, as soon as it had taken one bite the experimenter removed the carrot by pulling the string *ssss* (in Fig. 1), switched off the buzzer, and re-set the two-way key so that it was now impossible for the guinea-pig to obtain the carrot by turning its head. The time at which the buzzer was switched on, the time at which the guinea-pig made the required response, and the time at which the carrot was removed, were all recorded on the kymograph by the experimenter by means of the electromagnetic marker, although the second of these times could be obtained more accurately from the record of the animal's head-movements.

This procedure was repeated several times during the half-hour period for which the guinea-pig was in the stand. Some of the animals were given ten trials on each day, and for these animals the interval between successive trials was from 1 to 3 min., with an average of about 2 min. Other animals were only given five trials on each day, and for these the interval between trials was from 3 to 5 min., with an average of about 4 min. Irregular intervals of this kind were used in order to avoid the possibility that the guinea-pigs might form habits of turning their heads to one side at regular intervals instead of habits of turning their heads in response to the sound of the buzzer (cf. Pavlov, 1927, p. 41). The exact length of the interval was determined by the guinea-pig's behaviour, the buzzer being switched on (whenever this was possible within the limits of time allowed for the interval) at a moment when the animal's head was fairly still and facing approximately forwards. (It sometimes happened that on the first or second day of learning a guinea-pig took so long to make the required response to the buzzer that it was not possible to give the full number of trials during the 45 minute period of which a kymograph record could be taken.)

Altogether nine learning curves were taken, seven of them being records for 10 successive days, and the other two being records for shorter periods. In four of the curves the animal's head-movements were recorded on the kymograph, and in the others the times of response were measured with a stop-watch (cf. Section II). In the latter cases the experimenter was only able to note the times of response to the buzzer, whether before making the correct response the animal either made a partial turn in either direction or a complete turn in the wrong direction, the approximate times at which these turns occurred, the number of turns to either side during the intervals between trials, and any striking features of the animal's behaviour such as struggling, nibbling, or sleep.

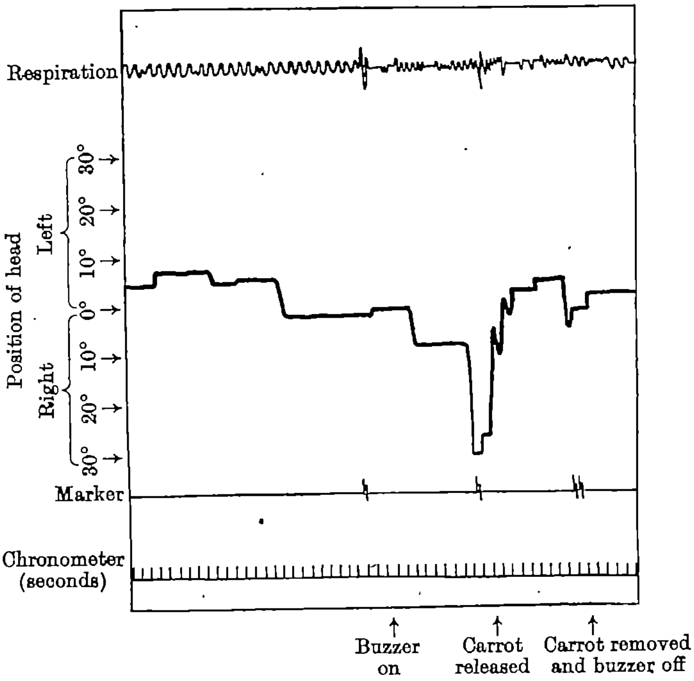

Fig. 2.

Fig. 2 is copied from a typical kymograph record of one trial, and it shows what measurements can be made from a record of this kind. The record shown is of the fourth trial on the third day of a guinea-pig which had ten trials each day, and in the figure the record has been corrected for the curvature of the path of the lever which recorded the animal's head-movements.

(c) *Reversal of habit.*

Some of the guinea-pigs which had learned to turn to one side on the sound of the buzzer were afterwards taught to turn to the opposite side instead. To produce this kind of 'reversal' of a habit the procedure was exactly the same as during the formation of the original habit; excepting that the guinea-pig now obtained the carrot by turning, on the sound of the buzzer, to the side opposite to that to which it had previously learned to turn.

(*d*) *Extinction of habit.*

With two guinea-pigs records were obtained of the 'extinction' (cf. Pavlov, 1927, p. 48) of a habit of turning to one side on the sound of the buzzer. After the original habit had been formed, *i.e.* after 10 days of learning, each animal was placed in the apparatus in the usual way, and in the usual state of hunger. The buzzer was switched on for 30 sec. at intervals corresponding to the intervals between successive trials in the original learning, but whatever response was made by the animal, the carrot was never released. This procedure was continued for 10 days.

IV. RESULTS.

(*a*) *Formation of habit.*

It was found that all the guinea-pigs subjected to the procedure described in Section III (*b*) gradually learnt to turn their heads to one side on the sound of the buzzer. That is to say, the time of response (*i.e.* the time between the moment when the buzzer was switched on and the moment when the animal turned its head to an angle of about 30° to the side on which this movement produced release of the carrot) became, on the average, progressively less in successive trials.

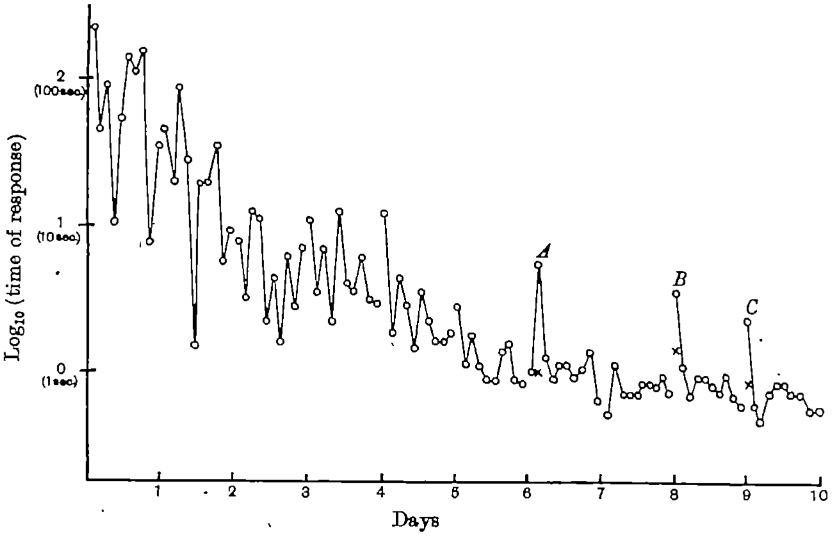

Fig. 3. A typical learning curve.

Nine learning curves were obtained in this way, and the curve shown in Fig. 3 is typical. In this case the animal was given ten trials on each day, and the head-movements were recorded on the kymograph. In Fig. 3 (and in the other learning curves in this paper) the times of response are plotted on a logarithmic scale. The average time of response diminishes from about 100 sec. in the first few trials to about 1 sec. in the last trials. But it will be seen that the curve is very irregular, although towards the end of the learning, when the average time of response is short, the curve is smoother. There are, however, in this part of the curve, a few points (*A*, *B*, and *C* in Fig. 3) at which the animal gave an abnormally long time of response; but in each of these cases the animal turned its head very nearly to the required position at the moments indicated by the crosses, and then returned to the forward position as though expecting the carrot to appear, and either waited for some time or repeated the partial response before making the complete response. Such partial responses, made a short time after the buzzer was switched on, occurred with all the guinea-pigs in very nearly every case where, towards the end of the learning, there was an abnormally long time of response. On the other hand, the long responses made during the first few days of learning were only occasionally preceded by quick partial responses of this kind.

The curve *AB* in Fig. 4 is an average learning curve for four guinea-pigs. The animals each had five trials on 10 successive days, and there was an interval of about 4 min. between successive trials. The times of response were measured by a stop-watch. The quantity plotted in the curve *AB* is the average logarithm of the times of response on each day, so that each point on the curve represents twenty readings.

The general behaviour of the guinea-pigs after the buzzer was switched on changed very much as the learning proceeded. At the first few trials the usual response to the buzzer was a slight 'start,' followed by a more or less prolonged period of immobility during which the animal appeared to be standing rigidly, with its eyes wide open. During this period respiration was rapid and irregular. After a time the guinea-pig would begin to move its head; the movements appearing to be, in some cases, struggles to escape from the apparatus and in other cases investigatory movements. Sooner or later the animal made a movement to one side sufficient to release the carrot, but this did not always happen until after a period of quiescence or sleep, and head-movements to the wrong side were often made before the correct response.. The time of response was thus quite unpredictable for the first few trials; for instance, while one guinea-pig at its first trial did not happen to make the correct response until the buzzer had been sounding for 35 min., another animal made the response in 28 sec.

Later in the learning the response to the buzzer appeared quite different. The guinea-pigs showed no signs of fear, but they sometimes showed food responses such as nibbling movements and sniffing when the

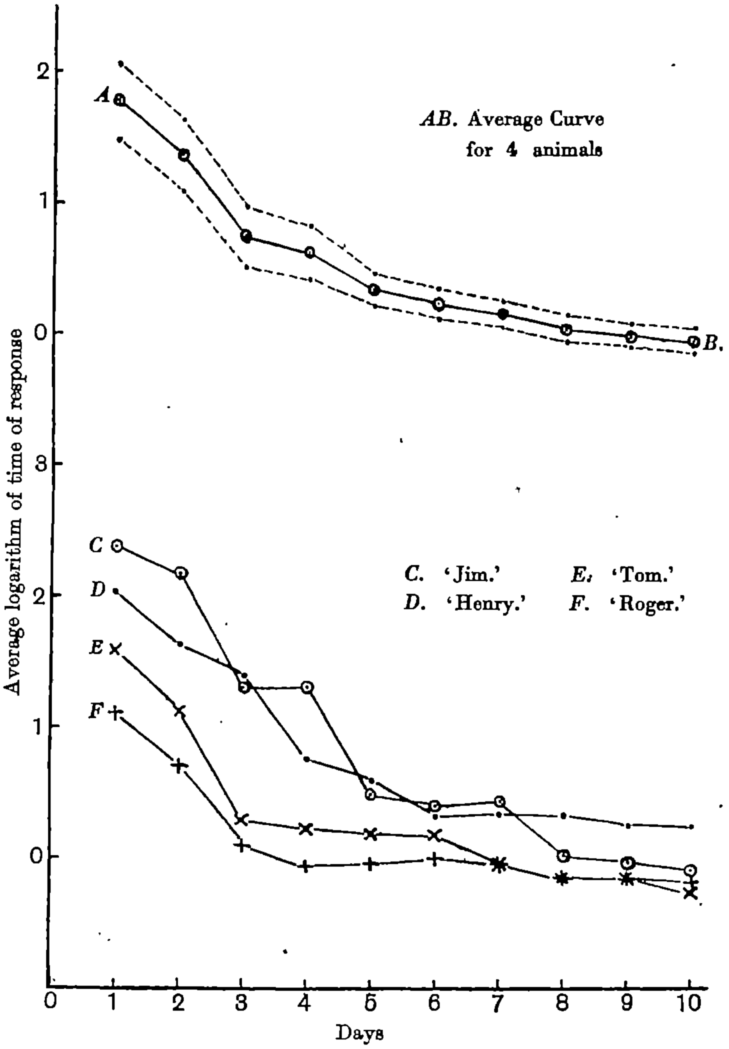

Fig. 4.

buzzer was switched on. They seldom or never turned their heads through any large angle to the 'wrong' side before making the correct response. Towards the end of the learning the animals would usually, after a short pause when the buzzer was switched on, turn their heads to the required position in one very rapid movement. If they did not turn quite far enough in the first movement they would sometimes turn further almost immediately, but in other cases there was a considerable interval before they made the complete response (as has already been indicated in connection with the points *ABC* in Fig. 3). Throughout the learning the guinea-pigs almost always ate immediately the carrot was presented.

(*b*) *Behaviour between trials.*

The animals differed considerably in their behaviour during the interval between successive trials. Some of them spent much of the time in quiescence or sleep; while others were usually active, either in moving their heads (perhaps in investigatory movements), or in nibbling the front of the platform. Violent struggles seldom occurred after the end of the preliminary training (cf. Section III (*a*)).

After 1 or 2 days of learning, when the animals were beginning to learn to turn their heads to one side—the 'food side'—on the sound of the buzzer, the frequency of head-movements to that side during the intervals between trials increased relatively to the number of head-movements to the other side. The movements to the food side were often followed by nibbling movements and sniffing, as though the animals expected the carrot to appear after the head-movement even though the buzzer was not sounding. Towards the end of the learning, movements to the food side during the intervals became less frequent with most of the animals; and this part of the guinea-pigs' behaviour may perhaps be regarded as an example of a process analogous to Pavlov's 'generalization' and subsequent 'differentiation' in the formation of conditioned reflexes (Pavlov, 1927, p. 110).

The average time between successive head-movements to the food side during the intervals was always (except on the first day or two of learning) much longer than the average time of response to the buzzer.

(*c*) *Changes in behaviour during each day.*

In Fig. 5 the curve *AB* represents the average values of the logarithms of the times of response in the five different trials in one day. The curve is calculated from the results of the four guinea-pigs whose average learning curve is shown in Fig. 4, and it refers only to the last 6 days of learning (*i.e.* to a period when the learning was fairly far advanced). The curve *CD* is identical with the curve *AB*, but it is depressed by an amount equal to the average improvement in the time of response from one day to the next during the 6-day period in question. It will be seen from the curves that there is a tendency towards 'regression' during the 23 hours between one experimental period and the next, in that the time of response tends to be longer in the first trial of any one day than in the last trial of the preceding day. The curves *EF* and *GH* show the average frequencies of head-movements to the 30° position on the food side and on the opposite side during the intervals preceding each of the five trials. It will be seen

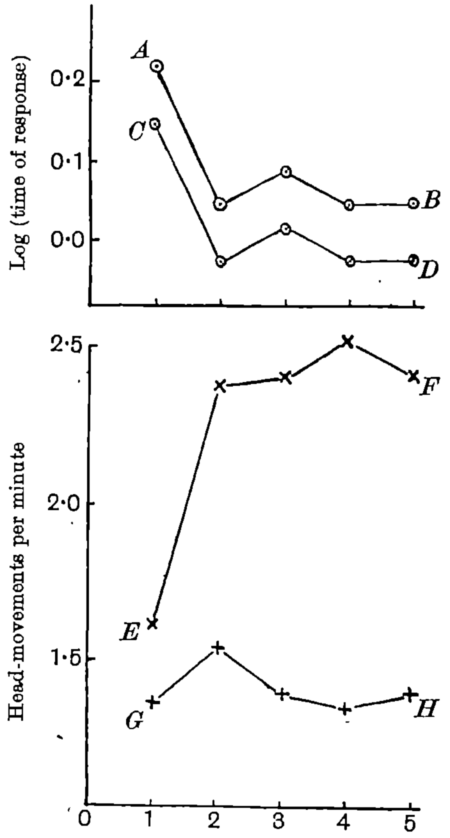

Fig. 5.

that head-movements to the food side tended to be less frequent during the interval before the first trial than during the subsequent intervals.

Both of these effects—the 'regression' in the time of response, and the tendency for head-movements to the food side to be less frequent before the first trial—were well marked in most of the nine learning curves. (Six of the animals showed a marked tendency towards regression, and eight showed the other effect clearly.) There are several possible explanations of these effects; for instance, they may be due to a state of fear which persists for some time after the animal is placed in the apparatus, or to an arousal of the animal's food-seeking tendency by the presentation of the carrot in the first trial, rather than to any property of the stimulus response connections underlying the habit of turning the head.

(*d*) *Variability of the results.*

The curves *C*, *D*, *E* and *F* in Fig. 4 illustrate the differences in the average performance on the first day and in the rate of learning between one guinea-pig and another. The four guinea-pigs represented by these curves were all males of about the same age (18 months); and their treatment, both during the preliminary training and during the learning was, as nearly as possible, identical. The number of animals (nine) used in the experiments was not sufficient to warrant any conclusions about the influence of such factors as age, sex, or the spacing of trials on the rate of learning; nor was it clear from the results whether the animals which were most active during the intervals between trials learnt more or less rapidly than the others.

The dotted curves above and below the curve *AB* in Fig. 4 indicate the variability of individual trials. The points on the dotted curves were obtained in the following way: A complete learning curve (similar to that in Fig. 4) was plotted for each of the four animals, and in each case a smooth curve was drawn through the points obtained. The vertical distance of each point from this smooth curve was then measured, and the average of these distances for each day was calculated for each guinea-pig, and for all four animals together. The vertical distances from the points on the curve *AB* in Fig. 4 to the points on the dotted curves above and below are *each* equal to the latter average distance. This means, roughly, that if *AB* were the smoothed learning curve for one animal, the chance (assuming a normal distribution of deviations from the smooth curve) that any individual reading would lie inside the area bounded by the dotted curves would be somewhat greater than the chance that it would lie outside them. It will be seen that the curves suggest that the variability of the times of response decreases (relatively to the average time of response, as well as absolutely, since the curves are plotted on a logarithmic scale) as the learning proceeds, but it must be remembered that the inaccuracy of the timing of the short responses (cf. Section II) makes the measurements of the variability unreliable towards the end of the learning.

(*e*) *Reversal of habit.*

In six guinea-pigs which had learnt to turn to one side on the sound of the buzzer, the habit was 'reversed,' according to the procedure described in Section III (*c*). It was found that in all these animals the earlier habit was eventually replaced by a habit of turning to the opposite side on the sound of the buzzer.

Fig. 6 shows a typical reversal curve. The curve is for an animal, 'Tom,' which had previously learnt to turn to the right, and its average times of response during the original learning are shown in the curve *E* in Fig. 4. Both in the learning and in the reversal the animal was given five trials on each of 10 successive days, and the reversal was begun on the day after the last day of the original learning. In Fig. 6 the circles show the times of response for the new habit (*i.e.* the times from the moment at which the buzzer was switched on to the moment at which the animal turned its head to the required position on the *left*—and so obtained the carrot), while the crosses show the times of response for the old habit (turning to the *right*) in those cases where this response was made before the new response. In some cases the animal made the old response several times before it made the new response, but in these cases only the time of the first old response is shown.

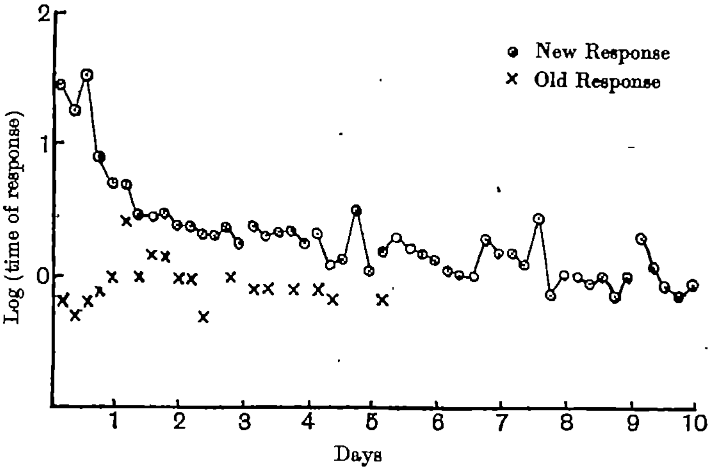

Fig. 6. A 'reversal' curve.

It was found that, as might be expected, at the beginning of the reversal the animals always gave the old response almost immediately the buzzer was switched on. After making this response they usually returned their heads to the central position and sniffed or made nibbling movements. This behaviour was usually repeated many times before the new response was made. The new response, when it occurred, was sometimes a part of a wild struggling movement (following upon failure to obtain the carrot by repeated turns to the old side), but in other cases it appeared to be an investigatory movement, or, possibly, a movement directed towards obtaining food.

In later trials the old response became less frequent and none of the four guinea-pigs of Fig. 4 ever gave the old response before the new response after the sixth day of reversal. During the reversal two of the animals developed a tendency to turn their heads rapidly, on the sound of the buzzer, first to the old side and then to the new side, but, after a few days of this behaviour, the turns to the old side were omitted.

The results give some evidence that the tendency to turn to the old side before turning to the new side is stronger in the first few trials of each day than in the last few trials of the preceding day. But the results are not sufficiently numerous to prove this.

(*f*) *Extinction of habit.*

With two animals curves of extinction were obtained, by the procedure described in Section III (*d*). In both cases the animal at first responded immediately to the buzzer, and made the response many times during the 30 sec. period for which the buzzer was sounding. As the extinction proceeded the average time of response to the buzzer became greater, and the average number of responses during the 30 sec. period became less. Later on, the animals usually failed to respond at all, and on those occasions when they did turn their heads while the buzzer was sounding they were almost as likely to turn to the opposite side as to the side on which they had obtained the reward during the original learning.

There was some evidence that the habit undergoing extinction tended to be stronger in the first few trials of each day than in the last few trials of the preceding day (cf. Pavlov, 1927, pp. 58–59).

V. DISCUSSION.

(*a*) *Simplicity of experimental conditions.*

In discussing the relevance of these results to theories (such as the conditioned reflex theory) which deal with the laws governing the formation of connections between stimuli and responses, it must be remembered that the ideals of physiological simplicity and exact repetition were not attained either with the stimuli or with the responses in these experiments. A buzzer, on any theory of hearing, gives a complex pattern of auditory stimulation, and it is not likely to make exactly the same kind of buzz at every trial. The movement of the head to one side involves a number of different muscles (though it is a fairly simple example of an integrated movement), and since the elevation of the head and the distance it was thrust forward varied somewhat in different trials, it is clear that the guinea-pig was not rewarded for exactly the same pattern of muscular contraction on different occasions.

Another point which must be remembered, both in connection with these experiments and in connection with experiments such as those of Pavlov, is that the apparent simplicity of the stimuli and responses is largely a result of the selective nature of the method of observation employed by the experimenter. When, in the present experiments, the guinea-pig turns its head in response to the sound of the buzzer, it must not be assumed that the auditory stimulus is the sole sensory stimulus tending to produce this response, or that the head-movement is the only response produced by the auditory stimulus. What actually happens, of course, is that the auditory stimulus produces a large number of responses—such as an increase in the rate of respiration, pricking up of the ears, and, probably, changes in circulation and in the tonus of many of the skeletal muscles. In many cases, no doubt, there were also actual movements of the animal's trunk and limbs, though these could seldom be observed. All these responses produce secondary, or 'backstroke,' sensory impulses, and the experiments do not show whether the head-movement is a direct response to the buzzer, or to the secondary impulses, or to both.

Nevertheless it may be claimed that the stimuli and response in these experiments were simpler and more nearly repetitive than those used in previous studies of 'trial-and-error' learning; and, until experiments are made under more strictly controlled conditions, it is reasonable to use these results as a basis for a discussion of the laws governing the formation of connections between simple stimuli and simple responses.

(*b*) *The conditioned reflex theory.*

The literature on conditioned reflexes does not contain many statements of the principle of conditioning which are sufficiently definite to be useful in a discussion of the theory that this principle will explain all the phenomena of learning. According to Holt (1931, p. 24) the passage in Pavlov's writings "most nearly approaching a definition" is (to quote only the part which is relevant to this discussion):

"If any casual (or 'indifferent') stimuli happen, once or a few times, to accompany stimuli which elicit definite inborn reflexes, the former stimuli begin of themselves to produce the effect of these inborn reflexes..." (Pavlov, 1923).

A better statement of the principle, since it includes the formation of conditioned reflexes of higher orders as well as the formation of first order reflexes, is that formulated by Holt (1931, p. 25):

"I. If one afferent (*i.e.* sensory) path already has connection with a motor path of discharge, then another afferent path if stimulated simultaneously, or nearly simultaneously, with the first afferent path will tend to acquire the same motor path of discharge.

II. After some repetitions of this simultaneous excitation, a stimulation of the second afferent tract alone will suffice to innervate the same motor path (*i.e.* to produce the same muscular contractions)."

Now it is difficult to see how the learning which occurred during the present experiments can be regarded simply as an example of the working of this principle. It is certainly true that some parts of the guinea-pig's behaviour (for instance, the nibbling movements which sometimes occurred when the buzzer was switched on) can be regarded as conditioned responses. It is possible, also, that during the original learning the formation of the connection between the buzzer and the appropriate head-movement was partly a process of simple conditioning; since, in every trial, the head-movement occurred while the buzzer was sounding. That is to say, the formation of the connection between the buzzer and the head-movement may have been partly a process which was due merely to the fact that in every trial the buzzer and the head-movement occurred in temporal contiguity—a process to which the subsequent presentation of the carrot was quite irrelevant. But the fact that the habit could be 'reversed' or 'extinguished' merely by altering the conditions under which the carrot was given, strongly suggests that the occurrence or non-occurrence of the reward after a head-movement had a more important influence than this simple process of conditioning.

The results seem to show that when a given head-movement (made while the buzzer was sounding) was followed immediately by a reward of carrot, this led to the formation or strengthening of a connection between the buzzer and the head-movement; but that when the head-movement was not followed by the reward the connection between the buzzer and the head-movement was weakened. The reward seems to act in a retroactive way, strengthening the connection between a stimulus and a response which occurred *before* the reward. An apparently retroactive process of this kind seems to be different from the processes of conditioning studied by Pavlov—the essential difference in the experimental conditions lying in the fact that in Pavlov's experiments the response (salivation) which is studied is one which is originally produced by the 'reinforcing agent' (food), whereas in these experiments the response (head-movement) is one which is not originally connected with the reinforcing agent, and which always precedes the reinforcement.

It is very difficult to see how the theories used by Pavlov to explain his results can, by themselves, explain these results. It is true that attempts have been made to explain learning involving apparent retroaction by a reward in terms of the formation of connections between groups of neurones (cf. Grindley 1927, Troland 1928), *i.e.* in terms of a mechanism of the same general nature as that employed in Pavlov's theories; but explanations of this kind always seem to involve a number of fresh assumptions in addition to the assumptions made by Pavlov.

(c) *The law of effect.*

There is, of course, nothing new in the suggestion that some other principle, such as that of the retroactive strengthening of connections by a reward, must be added to the principle of conditioning in order to explain the phenomena of learning. Long ago Thorndike thought that principles of both kinds were necessary, and his "Law of Use" is very nearly equivalent to the principle of conditioning, while his "Law of Effect" implies retroaction by rewards and punishments.

The present experiments seem to show that a principle involving retroaction of this kind is necessary for the explanation of the formation, reversal, and extinction of a relatively simple habit under the conditions which have been described. But if it is admitted that learning of this kind involves more than 'conditioning,' as 'conditioning' is defined by Holt, it may still be argued that the meaning of 'conditioning' should be widened, so as to make it include the retroactive formation of connections. This, of course, raises a purely terminological question. Perhaps it is most convenient to use 'conditioning' in the sense suggested by Pavlov and Holt, and to use some term such as 'retroflex action' (Troland, 1928) for the retroactive effect. The terms 'learning' and 'habit formation' can then be used to include processes of both kinds.

There are probably a number of ways in which 'retroflexes' resemble conditioned reflexes. Evidence has already been given that a retroflex action, like a conditioned reflex, is extinguished if the response is not reinforced; and it seems probable that such processes as 'external inhibition,' 'generalization,' and 'differentiation' occur in retroflexes in much the same way as they do in conditioned reflexes. It may be suggested, therefore, that retroflex action and conditioning are not two completely separate processes involving distinct physiological mechanisms; but rather that they are two different results of the working of the same physiological mechanism.

(d) *The form of the learning curve.*

When the experiments were begun it was hoped that with a relatively simple response and a relatively simple stimulus, and with no distraction by outside noises, smooth learning curves might be obtained; but the actual curves were nearly all very irregular. The general form of the curves suggests that during the learning process there was a tendency for the time of response to the buzzer to become *gradually* less, but that the time of response was also subject to rather large 'random' variations. None of the curves showed the sudden transition from a long time of response to a series of much shorter times which is supposed to be characteristic of 'insight'; but two curves showed a sudden and very marked drop in variability when the average time of response was about 4 sec.

Several attempts were made to explain the irregularities in the curves; or, rather, to find other features in the animals' behaviour which accompanied abnormally long or abnormally short times of response. It was found that, as might be expected, a guinea-pig which was 'sleepy' (as indicated by stillness of the head for some time before the buzzer was switched on, closed or half-closed eyes, and deep, slow and regular breathing) often gave an abnormally long time of response. But, apart from this, an examination of the records did not suggest any definite relationship between short or long times of response and respiration or head-movements during the interval before the buzzer was switched on.

It is probably not worth while to attempt to fit an algebraic equation to the smoothed learning curves obtained in an experiment of this kind; although it is tempting to argue that such an equation might represent a fundamental law of the formation of a retroflex connection between a simple stimulus and a simple response. But, while in these experiments the stimulus and the response may have been relatively simple, it seems unlikely that the curves represent merely the formation of a connection between a stimulus and a response by retroaction under constant conditions. It is probable that they also represent, to some extent, the gradual adaptation of the guinea-pig to the apparatus and to the buzzer.

Further, it is probable that the retroactive effect of the food is greater in the trials in which the time of response is short than in the trials in which it is long. All that it seems safe to argue from the curves is that, under the conditions of these experiments, the retroflex formation of a connection between a relatively simple stimulus and a relatively simple response is probably a gradual process.

VI. SUMMARY.

An attempt has been made to study 'trial-and-error learning,' when the response to be learnt is a relatively simple movement, and the stimulus for that response is also relatively simple.

Guinea-pigs, placed in a stand in a sound-proof room, were taught to turn their heads to one side in response to the sound of a buzzer, food being given as a reward when they made this movement. It was found that the average time of response became progressively shorter in successive trials. The habit could be 'reversed' by rewarding the animal for turning its head, on the sound of the buzzer, to the other side; or 'extinguished' by ceasing to reward the animal.

It is argued that it is difficult to explain these results on Pavlov's theory of conditioned reflexes; and that it seems probable that some other principle, such as that of Thorndike's "Law of Effect" or Troland's principle of "Retroflex Action," must be added to the principle of conditioning in order to explain the simpler forms of learning.

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A NOTE ON THE "BINAURAL THRESHOLD."

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When both ears are used in listening to a sound of given intensity, it is reasonable to suppose that an improvement in hearing will take place as compared with either ear used separately.

If we consider sounds of low intensity, however, *i.e.* sounds near the threshold of audibility, this statement requires considerable qualification.

In order to investigate the matter experimentally, a series of threshold measurements was made using a pure note of adjustable frequency.

The source of sound is a small moving coil loud-speaker driven by a heterodyne oscillator and amplifier. The output stage is shown in Fig. 1.

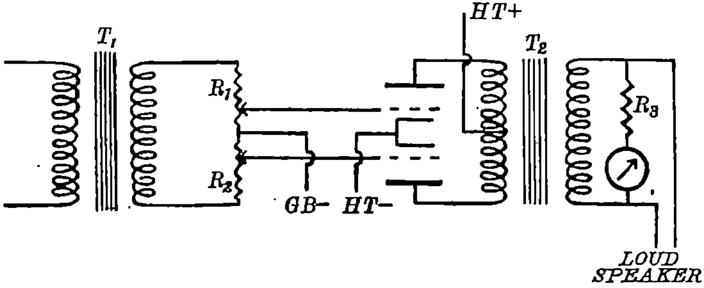

Fig. 1.

The secondary of the intervalve transformer T_1 is bridged by two resistances R_1 and R_2 of similar construction. Each resistance has a set of tappings so that the amplitude of the voltage variations impressed on the grids of the L.S. 5 A. output valves can be varied. The resistance steps of R_1 and R_2 are designed in such a way that they correspond to intervals of 5 decibels. For short they are called the decibel boxes.

By keeping the decibel boxes in step with each other, steps of 5 decibels are obtained, but by putting one box 5 decibels above the other we can obtain steps of half this, with sufficient accuracy for all practical purposes.

The output transformer T_2 supplies current to the loud-speaker, in parallel with which is a circuit consisting of a resistance R_3 and an A.C.

rectifying microammeter. This instrument is used as a control on the intensity of the oscillations produced, all results being reduced to a standard reading.

The loud-speaker *S* (Fig. 2) is placed in a sound-proof box *B*, the tube *T* being inserted to convey the sound to the ears. An acoustic filter *F* is placed in this conduit to remove any residual harmonics. The tube then branches as at *P*, each branch terminating in ear-pieces like those of a telephone receiver; these are much more comfortable than stethoscope ear-pieces. Part of the tubes are made of rubber so that either ear can be cut off from the sound by means of a clip.

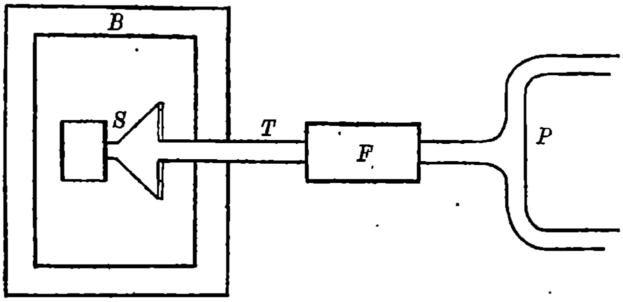

Fig. 2.

The loud-speaker and associated apparatus are placed in a sound-proof room. A system of flash-lamp bulbs enables the subject in the sound-proof room to communicate with the experimenter controlling the apparatus outside.

The following experimental observations were made for various frequencies:

(1) The reading on the decibel boxes was taken for the threshold of the left ear, no sound entering the right ear.

(2) The reading on the decibel boxes was taken for the threshold of the right ear, no sound entering the left ear.

(3) The reading on the decibel boxes was taken for the threshold of both ears taken together.

A blank experiment was performed with the tubes leading to both ears blocked; it was found that the sound had to be raised in intensity by about 50 decibels before it could be heard. This shows that stray sounds from the loud-speaker not passing through the tubes could be neglected.

Six sets of observations were made as above for each frequency. It was found that for any given frequency the left ear sometimes had a lower threshold than the right, while at other times the right ear had a lower threshold than the left. This is to be expected, since the threshold will depend on the actual fitting of the ear-pieces; we must consider these as part of the mechanism of the ear for sensitivity purposes.

Furthermore, the sensitivity of either ear will vary from frequency to frequency, so that the more sensitive ear may for some frequencies be the left and for others the right.

Table I was derived in the following way. The difference in decibels between the threshold of both ears together and that of the more sensitive ear, *i.e.* the ear with the lower threshold, was found for each separate group of three observations. The average differences for the six sets of observations are shown in column 2. The same average differences between the less sensitive ear and both ears together are shown in column 3.

Table I. *The frequencies are in the order in which the experiments were performed.*

Frequency	Decibels	Decibels
500	5·8	12·5
400	3·3	8·8
300	0·8	2·1
250	−0·8	3·8
200	5·1	10·8
180	2·9	7·5
160	2·9	5·8
140	−1·7	0·8
120	0·0	2·9
100	0·4	3·3
600	0·4	2·9
800	2·9	5·8
1000	−1·7	4·6
1200	0·8	7·1
1400	3·3	7·5
1600	−1·7	6·7
2000	−1·2	5·0
2400	−1·2	2·9
3000	0·0	6·2
4000	0·8	3·8
6000	0·0	8·3
8000	1·7	4·1
	22·8	123·2
Average	1·0	5·6

From the average of all these observations it can be seen that the increase of sensitivity obtained by using two ears instead of only the more sensitive ear is 1·0 decibels, an amount hardly outside the experimental error. On the other hand, the improvement of both ears over the less sensitive ear is considerable, being 5·6 decibels.

It follows that the term "binaural threshold" can have no exact application. Also such statements as "two ears are better than one but not twice as good" have very little quantitative foundation, at any rate when we are considering weak sounds, and this is just when increase of total sensitivity is of value.

What we can say is that for pure tones near the threshold of audibility two ears will in general be much better than one if that one is the less sensitive ear, under the conditions existing at the time for the particular note concerned, but that two ears will be no better than one if that one is the more sensitive ear.

If the sounds are not pure, and especially in the case of "noises," it may well happen that the right ear is the more sensitive for some of the components and the left ear more sensitive for others. In such a case it will therefore be true that "two ears are better than one."

Another point is that for pure tones binaural localization is impossible when the threshold of the less sensitive ear is approached; this means that the sound can still be heard below this intensity but cannot be located. Overall amplification of the sound would be of value in such cases as this.

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MEDICO-PSYCHOLOGICAL ASPECTS OF NORMALITY[1].

By EDWARD GLOVER.

I. *Historical: the introduction of psychological standards* (pp. 152–3).

II. *Working psycho-analytical definition: difficulties and objections* (pp. 153–5).

III. *Systematic approach* (pp. 156–62).

  (*a*) *Clinical* (pp. 156–8).

  (*b*) *Structural* (pp. 158–60).

  (*c*) *Economic* (pp. 160–2).

IV. *Adaptation factors: real and unreal anxiety* (p. 163).

V. *Summary of definitions* (pp. 164–5).

VI. *The illusion of absolute normality: its significance* (pp. 165–6).

I. HISTORICAL: THE INTRODUCTION OF PSYCHOLOGICAL STANDARDS.

ALONGSIDE the immunities conferred on medical practitioners by Act of Parliament, there run certain unwritten privileges of a unique order. Perhaps the most important of these is the right of each practitioner to establish a standard of normality for his patients. Had this been restricted to the sphere of organic medicine, to distinguishing between physiological and pathological function, the privilege might well have gone unchallenged. What ultimately undermined the doctor's authority was the invasion of his consulting room by an expectant file of psychoneurotics; these made the most convincing show of organic illness, yet remained in many cases refractory to every nostrum. In face of this reduction of authority and laceration of professional pride, the physician was quick to take steps to repel the invasion. He had two alternative plans of campaign, one conscious, the other unconscious. The unconscious plan consisted in falling in with the patient's belief in his own illness and exacting revenge by a series of painful and sometimes humiliating retaliations. He drew the dyspeptic spinster's teeth, removed the unconscious homosexual's appendix, scoured the conversion hysteric's uterus, purged the latent paranoid hypochondriac's bowels and injected all and sundry with vaccines or glandular extracts. The alternative and conscious plan was to oppose the patient's view that he was sick, convince him of this palpable untruth and return him to his friends rejoicing or lamenting, as the case might be. Both of these methods are essentially suggestive in nature.

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[1] A popular lecture delivered before the B.P.S. on February 29th, 1932. It is not generally intended to publish such lectures, but an exception is made in this instance by special request and in view of the widespread interest in the topics discussed.

But in this neurotic combat, the doctor's victory was never very secure; it was gradually turned into a partial defeat by the superior enlightenment of the lay public. Modern psychotherapy owes a great deal of its authority to the fact that in psychological respects, patients are more up-to-date than their doctors. Anyhow, for good or ill, psychological medicine became a speciality, and with the formation of this new branch it was no longer necessary to eject psychological cases on to the pavements of Harley Street in a state of suppressed fury and disappointment. But by the same token *the old organic standards of normality went by the board.* Their disappearance was accelerated by the increasing demands made by the lay public. The obsessional neurotic with his peculiar mental constructions had followed in the footsteps of the hysteric, the borderline psychotic had followed the obsessional, but now the unhappy and the maladapted, dejected husbands and dissatisfied wives, and every sort of idiosyncratic character type, all crowded into the consulting room demanding attention.

In the face of this fresh invasion, the courage of the new medico-psychological specialist failed him; and he began to copy the tactics of his predecessors. Unhappy and incapacitated cases who could not produce a respectable psychological symptom narrowly escaped being called social malingerers. And once more Harley Street resounded with a hum of admonition punctuated by firm slamming of doors. In course of time some of these therapeutic outcasts were treated by psycho-analysis, a method that had already been found useful in hysteria and obsessional neuroses. The results of these early experiments were patchy. Some remarkable successes were offset by minimal improvement in other cases, in spite of years of patient work. Nevertheless, the amount of success was such that *it was no longer possible to exclude a capacity for personal happiness and peace of mind from the professional categories of normality.*

II. WORKING PSYCHO-ANALYTICAL DEFINITION: DIFFICULTIES AND OBJECTIONS.

To round off this compressed history; the psycho-analyst has found himself compelled, owing to the nature of his case material, to include amongst the standards of *abnormality* not only bodily symptoms and psychic symptom constructions, but various social inhibitions and degrees of maladjustment in work and love. He stipulates, however, that these maladjustments should be accompanied by sufficient affective discomfort to drive the patient to consultation. In so doing he has committed himself to a tentative definition of *normality*. As I expressed it once "a normal individual is anyone who is free from symptoms, unhampered by mental conflict and who shows satisfactory working capacity": to complete this definition I would now add "and who is able to love someone apart from himself."

Now the flaw in this system of definition is obvious: I cannot add the rider that anyone who does *not* come to consultation is sufficiently happy and well adapted to be called normal. There are innumerable social occasions when casual observation compels us to recognize the apprehensions, eccentricities, inhibitions and symptom formations permeating everyday life. Perhaps the most impressive example is that organized 'health movement,' which seeks to canonize anxiety reactions as part of a praiseworthy hygienic system. It is not simply that our advertisement columns encourage intestinal hypochondria by the terrifying caption 'Constipation,' or that we are exhorted to purify the blood, to kill flies, to wear soft collars, expand the chest and breathe pure air, use carbolic soap or drink hot water: the fact is we are encouraged to invest every round of waking activity with a typical neurotic or psychotic anxiety charge. And the man in the street enters into this fearsome conspiracy with feelings that range from intimidated acquiescence to almost religious enthusiasm. I shall not labour the point farther, merely asserting that this inculcation and expression of mass anxiety can be tracked down to individual anxieties, in fact that within the range of socially accepted normality, we can detect reactions which, were they manifested during an analysis, would be regarded as 'abnormal.'

Now even if the man in the street agreed that some of his emotional phases might be regarded clinically as exceeding a normal standard, he would doubtless repudiate with warmth the suggestion that many of his most cherished habits are simply unlabelled symptoms. He would suspect and with good reason that the sanctuary of his private character was threatened. To any such protest the medical psychologist's reply would be purely professional, viz. that the character of the man in the street is quite safe from professional inspection so long as the man remains in the street. The mere fact, however, that such acute differences of opinion might arise, compels us to examine carefully all professional standards of normality.

Curiously enough a more urgent reason for investigation seems to have escaped the notice of the people most concerned. Possibly theologists, metaphysicians and sociologists are too busy with their routine work to observe that the frontiers of their sciences have been violated. Yet the appearance of the medical psychologist with his inquisitive tendency to measure happiness and peace of mind, his preoccupation with conscience and crime, his concern with categorical urges, inhibitions and the phenomena of 'will,' is at the least something of a portent. And it is incumbent on the medical psychologist to justify his intrusion.

To a certain extent he has already done so; but his favourite method has usually been regarded as begging the question. He has naturally been concerned with the patently abnormal and has applied his discoveries in this field to the problem of alleged normality. For example; when the theologist has said that human guilt and original sin belong to his particular province, that happiness and misery are essentially theological measures, the medical psychologist has usually responded by producing a sheaf of case notes. Fortified by his most recent interviews with a case of melancholia or mania or manic depressive insanity, he has pointed ou that the melancholiac feels and appears the incarnation of misery, that the expansive hypomanic greets us with the sunny smile of happiness. He may go further and claim that the manic depressive psychosis is essentially a conscience phenomenon, an alternating hypertrophy and atrophy of unconscious conscience. Some of these facts are not denied by lay psychologists and are accepted even by enlightened laymen; yet any attempt to apply them to normal affective states is liable to be regarded as a *non sequitur*.

This criticism is no longer valid. The psycho-analyst has for some years back collected and marshalled his data over a wider front. Not only has he investigated the mental structure and function of a great number of cases of emotional maladaptation unaccompanied by neurotic symptoms, he has analysed groups of alleged normal individuals, of any age from about 2½ years to over 60, taken from different social and cultural levels and from practically every civilized race. He has, moreover, specialized in the transitory phases of abnormality to be detected in early childhood. And even at this early stage of his investigations he is able to formulate preliminary generalizations, some of which are already familiar, others quite unexpected.

III. SYSTEMATIC APPROACH.

(*a*) *Clinical.*

Starting with the descriptive aspects of human normality, it may be of interest that the most difficult person in the world to analyse completely is the ordinary so-called 'normal' person. Nor is this surprising observation to be explained by mere perversity. The 'normal' person resists all affective examination of his stability for a very natural if no longer justifiable reason. He resists it because his 'normality' represents a victory over an original state of madness. We no longer regard the psychoses and neuroses of adult life as spontaneous pathological disruptions and distortions of a normal life. One of the earliest and, at the time, almost incredible psycho-analytic formulations was that which ran, "no adult neurosis without an infantile neurosis." Already that has become a mere platitude compared with the newer and more far-reaching conclusion, "no adult normality without an infantile psychosis." The formulation is not entirely original: it has been put forward at various times by the more intuitive of our writers. To quote a recent example: the author of *High Wind in Jamaica* in a curiously technical digression remarks concerning the minds of children that they "are not just more ignorant and stupider than ours, but differ in kind of thinking (are *mad*, in fact)." He goes on to say regarding the mentality of infants, "one can no more think like a baby, in the smallest respect, than one can think like a bee." And although this second pronouncement is completely wide of the mark, the former is a sound intuition. The professional psycho-analyst has reduced these and similar intuitions of naïve psychologists to an orderly systematization. Every child in the first year of life is from the adult psychiatric point of view in a state of panpsychosis always acute and very largely hallucinatory. Between the ages of 1 and 2½ his mentality alternates between an introjective psychotic pattern (schizophrenic type with or without melancholiac elements) and a projective psychotic pattern (paranoid type). Assuming successful struggle with this phase, he emerges between 2½ and 4 with patterns of an infantile obsessional type and this phase overlaps a little with a phase of infantile hysterical reaction and phobia formation between the ages of 3 and 5. These may be regarded as the *normal psychoses* and *normal neuroses* of childhood, but any accentuation of the anxiety appropriate to one state may bring about a *clinical* infantile psychosis or neurosis; and naturally any persistence in later infancy of characteristics more appropriate to an earlier stage is also to be regarded as pathological for the later stage. There are three main reasons why these earlier psychotic patterns have not been recognized. The most superficial is that we naïvely expect the 'psychotic' reactions of a child to be identical with the clinical psychotic reactions of an adult: actually adult psychoses show certain repair aspects which are developed in response to a highly specialized environment. The second reason is more interesting to us. We ignore the 'psychotic reactions' of childhood because it would be intolerable to reanimate stages we have ourselves passed through and overcome. So we term them the 'normal reactions' of a child, even when they are plainly of a psychotic pattern. If a child rushes into our arms because it is suddenly terrified of a bundle of clothes, we are unperturbed. If an adult does the same, we gravely consider his 'certification.' The third reason is as comprehensible as the second if a little less flattering to our self-esteem. Having brought the infant into the world, we find its claims on our adult endurance too persistent and exorbitant, and are only too glad when it appears to change from a red-faced brat squalling in ectoparasitic need, to an angel face of two or three displaying endurance that entitles it to enter the Kingdom of Heaven. So we are not too ready to recognize the fact, if and when this sudden paleness of character indicates an internal drenching psychic haemorrhage[1].

To return to the child of five: from then on to puberty there is apparently a lull. And in so far as it is a true lull, implying a reduction in the intensity of primitive infantile instinct, it is a well-earned rest from the anxieties and unconscious guilts of childhood. But the respite is largely a matter of surface calm. Actually the child is busy consolidating the barriers it has already thrown up against the intolerable anxiety of frustrated instinctual drives. Day after day it seeks to entrench itself in a more reassuring reality, supporting itself the while on affectionate relations with its family, the aim-inhibited residue of more tempestuous claims. And there is not too much time to lose. The child's defensive rampart will soon have to weather the last great thrust of instinct which approaches with puberty. It is not difficult to imagine the possible course of events after the first shock of puberty. If the defences break down, two possibilities are open. One individual may sooner or later develop a neurosis or a psychosis: in other words by sacrificing certain ego functions, a degree of normal relation to reality is preserved (except, of course, in the case of total regression). Another may escape open neurosis and psychosis, but he does so at the cost of a sub-acute inflammation of all his relations to reality, particularly his social contacts. We then say for purposes of description that he has developed a major characterological defect.

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[1] In such a brief survey, a certain amount of over-emphasis may be permitted. It is only fair to add, however, that during these 'normally psychotic phases' the average child develops a degree of adult organization with which it is seldom credited. The latter enables us to distinguish between the 'normally psychotic' and the 'clinically psychotic' child. Interesting parallel observations may be made in the field of anthropology. The 'normal' primitive tribesman combines a remarkable degree of reality organization with systems of a frankly psychotic and neurotic type, *e.g.* paranoid fears (animism) and obsessional systems (magic).

Should, however, the defences hold, all we can detect is a phase of volatile emotional reaction corresponding with the years of adolescence; this is followed by more or less permanent stability, only occasionally shaken by adult events of psychic importance: births, marriages and deaths, or successes and failures in career. In other words under the immediate internal impact of adult genital primacy, the ramparts of child defence, although violently shaken, remain intact and apparently solid.

Now we might say that for the adult this stable rampart system represents normality; that apart from a little cultural French polishing, adult normality is the end-product of child defence. But if we test this general formulation by clinical inspection of the population at large, we are once more brought up short by the number of idiosyncrasies of habit, minor symptoms of an anxiety and obsessional type to be detected in so-called normal people. And that is not all. There are times when the ramparts are so hard and enclosing that they appear to function as a mausoleum. The individual is entombed within his rigid character. The voluntary celibate, the sterile but compulsive work fiend, the shilly-shally, the miser, and the compulsive criminal, may be cited as illustrative types. Even a quantitative standard does not solve our difficulty. We might say that so long as the mausoleum type is not a nuisance to himself or to other people and neither comes nor is driven to consultation, he can be called normal. But that is only to say that socially it is not our business to interfere. If we permit ourselves the lesser interference of thinking about human behaviour, we may pass the self-appointed bachelor or spinster or even the work maniac and the doubter without comment, but we can scarcely label the miser normal without further investigation.

(*b*) *Structural.*

It will be apparent that a purely descriptive approach does not allow us sufficient elbow room for measurement, so we must turn our attention to the structure and function of the ego as a whole. Making use of spatial conventions, the psycho-analyst tries to express his views of the ego by calling it a psychic organ, and still further a surface psychic organ. He fixes it in psychic space by reference to perceptual consciousness. The ego is developed round perceptual consciousness. It is a surface organ with at least two facets, one turned towards the external stimuli of the outer world, and the other turned towards the instinctual stimuli of the inner world. The instincts are represented by derivatives, either in ideational form or in the form of affects. The primary function of the ego is to assist in the reduction of psychic stimulation; and this in effect means that the ego must cater for the gratification of inner needs in so far as these can or may be gratified in the external world. If instinct cannot or may not be gratified in the external world or on the person's own body, the ego must undertake the task of mastering instinct. This mastery can be achieved in numerous ways. One is the way of repression, which if successful prevents ideational representation in consciousness and prevents the development of affect. Another way is the way of reaction formation. This is a kind of counter-charge, wherewith tendencies are reinforced which nullify or inhibit the tendencies of primitive impulse. Still another is the mechanism of sublimation whereby primitive energies are deflected from their original aim and gratified in other directions. There are many others, but taking the three I have mentioned it is to be noted that repression is silent, invisible and incalculable; reaction formation is noisily obvious; and sublimation, though obvious in end results, is much less obtrusive in operation. Now if we are going to judge normality solely by the noisier reaction mechanisms (that is to say, apply mainly characterological standards), we neglect the fact that none of these mechanisms can master instinct independently. We neglect the compensating and balancing tendency of the ego and in particular we do less than justice to the factor of repression. From this point of view our miser, though open to the charge of abnormality in a characterological respect, might plead that in the sense of ego function he was perfectly normal, that he felt perfectly comfortable in his adaptation, and finally that it was none of our business. And unless his miserliness took a form that justified social interference—such as collecting dangerous micro-organisms or high explosives without adequate precautions—we would be compelled to apologize and beat a retreat.

In passing we learn something here of the animus behind characterological studies. Fundamentally we are interested in character for self-preservative reasons: it pays to be able to estimate quickly the instinctual tendencies of our neighbours. Characterology provides us with a quick 'spotting' system for this purpose. But 'good' or 'bad' categories apart, we have a special reason for being suspicious of the terms 'normal' or 'stable' character. If we find that our neighbour is not dangerous, we are apt, partly from relief and partly on the principle of 'live and let live,' to turn a blind eye upon his peculiarities. In short, the concept of character is a legacy from naïve, conscious, self-preservative psychology for which there is little use in metapsychology. Some years ago I endeavoured to define it as an organized series of behaviour reactions promoting a stable equilibrium between instinct tendencies and reality gratification, adding that these reactions produce a more or less harmonized adaptation through the mechanism of displacement. I believe this definition may still be of service in estimating normal aspects of character, but it must be admitted that characterology is one of the most lop-sided of psychological pursuits.

The argument so far amounts to this. Descriptive standards of normality, taken from behaviouristic psychology, are too narrow. An examination of the structure of the ego is a little more satisfactory, because it gives us a better grasp of the individual aspect of normality, viz. *a psychic organ in successful function.*

But we are still hampered by the fact that a large part of the ego is unconscious and, unless steps are taken to remove the barrier of repression, remains permanently unconscious: we have no alternative therefore but to examine more closely the economic aspects of psychic function.

(c) *Economic.*

It has been indicated that the function of the ego is to reduce psychic tension to an optimum level: that whatever instinct barriers are erected for this purpose, they should not impede the ego in legitimate adaptation. We can express this in another way by saying that the psyche is developed to deal with quantities of excitation, and that it does so by virtue of its conscious and unconscious appreciation of certain other quantities, viz. quantities of anxiety. For purposes of simplification, I shall include in the term anxiety that special form which by reason of its endopsychic conditioning is thought of as guilt. *The pleasure-pain principle then is seen to be the most primitive measure of normal reaction for the infant; but it cannot become an adult measure until it has undergone that slow process of modification during childhood which converts it into the reality principle.* So far the prospects of accurate definition appear rosy. But the question arises: can we regard the establishment of the reality principle as an absolute measure of adult normality? Presumably we ought to stop at this point in order to define reality: I propose, however, to approach this definition in a roundabout way. For the moment I shall simply state the conclusion that *in the present state of Western civilization we have no grounds for assuming that normality and complete reality adaptation are identical. Grattez l'homme et vous y trouverez l'enfant.* As the sole criterion of adaptation, the adult reality principle is inadequate: the adult must have a certain capacity for exploitation of earlier pleasure systems. But that exploitation must not induce an intolerable charge of anxiety. In other words, the two systems must not act in complete opposition. They must be mutually complementary: the pleasure system must learn to be content with the exploitation afforded by adult reality, and adult organization must be sufficiently porous to permit the safe expansion and discharge of infantile vapours.

Now in the course of examining the most primitive mechanisms set in motion by the 'pleasure-pain' principle, we make an astonishing discovery: a discovery that has considerable bearing on definitions of 'normality,' to say nothing of definitions of 'reality.' Earlier in this presentation I described the first phases of childhood in terms of psychotic reaction and indicated a developmental series of psychoses and neuroses preceding the 'normality' of later childhood. And it may have occurred to the reader that unless this description is altogether fantastic, there must be many grades of reality differentiation. This inference can be supported by the psycho-analysis of children, normal adults and psychotics. The child sets out on its developmental journey equipped with an intolerance of anxiety which it proceeds to turn to biological advantage. When, for example, an infant suddenly turns puce, alarmed onlookers hunt for a crumb or a pin or a wet napkin: when it yells, those within earshot run to its assistance. Failing all help, the child tends to pass quickly into exhaustion and, if really deserted, would whimper its way to sleep and ultimately die. But the spirit of the age is on the whole against infanticide by neglect. The external world frantically conspires to make environment approximate to the child's demands for immediate relief. And the child responds by loving the most ready part of environment. To cut a long story short the result of these experiences is to bring about a confusion of the external and internal world. The child tends to regard the pleasure-producing environment as part of itself and tries to treat internal pain as a foreign body. But the main source of psychic tension and 'pain' is frustrated instinct and in particular frustrated aggressive instinct. When, therefore, the child tries to rid itself of this painful stimulus, it employs the most primitive mental mechanism of projection.

It projects its painful instincts into the external world, which in turn is felt to be dangerous and painful. Admittedly the outer world is dangerous and painful in many respects. But it is seldom dangerous in the projected respects. A father may be cruel and stern, but he is not as a rule a dangerous homicide. A mother may be vexatious or severe, but she is not a devouring animal. Yet phobia after phobia proves that parents are so regarded in the unconscious layers of their children's minds. Out of the frying pan into the fire: projection tends to defeat its own ends and so a vicious circle is established. The more frustration, the more hate: the more hate, the more dangerous the external world: the more dangerous the external world, the more inhibition: the more inhibition, the more frustration. And so it goes on. This vicious circle is broken in two ways: first, by the auxiliary mechanism of primal repression and secondly, and this is what concerns us now, by an exploitation of 'real reality.' The more terrifying the phantasy external world, the more reassuring the actual outer world. So the hunt for reality commences. In place of the phantasied dangers, there is in some cases a positive welcoming of reality dangers and in most cases a fuller exploitation of the actual love, affection and protection afforded by family environment. Whether the original projected system is ever completely abandoned is questionable. So far analysis of the normal adult shows that his psychotic mechanisms still exist but with such a weak charge as to be negligible.

Turning once more to the task of definition, we are compelled to conclude that at present, at any rate, it is not within the scope of metapsychology to give a definition of 'absolute reality.' In spite of this we are able to attach a precise significance to the term 'reality-testing.' The metapsychological definition of 'reality-testing' is based on the view that the ego is a sampling apparatus as well as an organ for reducing instinctual excitation. *Reality-testing then, from the medical psychologists' point of view, is the capacity to maintain psychic contact with the objects which promote gratification of the modified impulses of adult life. And adult normality is a state in which infantile psychotic views concerning the external world have been so reduced that they do not interfere with possibilities of adult gratification; in other words it is a state in which the psychotic estimate of the object world coincides with an objective estimate in two main respects, (a) the amount of love that can be satisfied and (b) the amount of danger to the ego that is present.*

IV. ADAPTATION FACTORS: REAL AND UNREAL ANXIETY.

Once again, however, our hopes of an absolute definition of normality are dashed. Examination of our now familiar friend in the street shows that on many occasions it is difficult to distinguish between real and neurotic anxiety. Run a foot plank along a pavement and most people will walk it with ease and pleasure: run it across a shallow stream and quite a few adults will baulk: run it across a deep crevasse and except in situations of extreme urgency baulking will be regarded as common sense, that is to say, a normal reaction.

It is sometimes equally difficult to distinguish between real anxiety and psychotic anxiety. I have already hinted that some 'modern' health movements are in many respects manifestations of psychotic reaction. Systematic examination would doubtless uncover many others. When, after looking at a landscape, an individual experiences surpassing peace of mind and a deep sense of communion, our investigations are rather hampered by preconceptions of aesthetics. But when another develops a deep feeling of depression and sense of guilt at the sight of a rain cloud in a distant sky, we are entitled to suspend aesthetic judgment and enquire whether this is not an occult paranoia. The list might be continued indefinitely. How much of the peace of mind of seemingly well-adjusted individuals is due to their symbolic preoccupations and satisfactions? How much primitive fetichism is represented by the 'hotis' and 'netsukis' of the collector's cabinet, the china dogs of a nineteenth-century kitchen mantelpiece, the treasured editions of the bibliophile, the favourite conceptual preoccupations of the metaphysician or the record vegetable marrows of the allotment holder?

Anyhow, the fact cannot be concealed that with every attempt to establish an absolute standard of normality, we encounter the same difficulty, viz. that there is no sharp edge to the definition. It is invariably blurred by a number of obvious exceptions. There is, however, one point to be noted about the exceptions: the individuals in question rarely come to medico-psychological consultation. And this implies either that the activities are playing a part in the balance of individual adaptations, or that the amount of associated tension and anxiety has not become unbearable. Besides, if we find that the exceptions to a number of standards are invariably of the same type, there appears to be no reason why we should not overcome this difficulty by incorporating the exceptions in a fresh definition. In short *the exceptions to individual standards compel us to introduce into the definition a crude factor of social adaptation.*

V. SUMMARY OF DEFINITIONS.

We are now in a position to assemble some rough generalizations concerning the medico-psychological aspects of normality. From the descriptive point of view the most superficial estimation of normality is made by reference to the presence or absence of symptoms, characterological defects and maladaptations. Taking a broader view, normality can be subdivided into a number of reaction systems, each of which is appropriate to a particular phase of instinct mastery. These systems are in the first place psychotic, later neurotic and in adult life characterological. They represent ramparts created under the stimulus of anxiety. The most obvious normal rampart is adult character. Character is a series of behaviour reactions promoting a stable equilibrium between instinctual demands and gratification in reality, using for this purpose mainly the mechanism of displacement. This particular standard of normality is inadequate, (*a*) because character is mainly a (pre)conscious end-product, (*b*) because it is itself liable to deep pathological changes which may go undetected. Taking a wider view of the ego, normality implies simultaneous yet harmonious function of a number of mechanisms, (*a*) for controlling instinct and (*b*) for securing direct (or substitute) instinct gratifications. The ego normally acts as a compensating balance. The most incalculable and elastic factor in this aspect of normality is the mechanism of repression. A still broader view of normality is that it represents a harmonious confluence of the reality principle with the primitive pleasure principle. At this point a more cautious definition is necessary: normality is possibly an approximate overlapping of psychotic and objective reality-testing. An alternative view is that normally the anxiety precautions evoked by reality are sufficient to cope with co-existing psychotic and neurotic (unreal) anxiety systems.

This last formulation is perhaps the most important of those I have put forward and.it may be well to underline it by additional illustration. It is true that when the ordinary man catches a bus we do not call for his certification as a lunatic. Nor do we certify the hysteric when, dashing into the traffic, she narrowly escapes being run over. The poet may sing of Phoebus Apollo with complete immunity: and the artist, if he chooses, is safe to paint the wheel of life in tartan colours or with eccentric axle-hubs. But we *do* certify the melancholiac when she threatens to throw herself under a real bus; we do certify the paranoiac when, having affirmed that all busmen are trying to run him down, he attempts to set bus depôts on fire; and we certify the schizophrenic when he begins to see Juggernaut coming down Bond Street. Nevertheless the ordinary man may owe his freedom from incarceration to a happy merging in time and space of real and lunatic interest in omnibuses. To repeat: *normality may be a form of madness which goes unrecognized because it happens to be a good adaptation to reality.*

The task remains of reconciling these more or less absolute standards with the fact already stressed, namely, that, judged by all of them excepting possibly the last, normality is non-existent. I have already suggested that we can get over this difficulty by introducing a crude social factor, viz. a social standard of adequate adaptation. By so doing we can artificially increase the normal range of characterological and emotional reactions and at the same time sanction the exploitation of minor symptom formations, provided the latter are not disruptive in individual respects or offensive in conventional (social) respects. As a matter of fact such a standard is not so crude and artificial as it appears. It corresponds to an individual factor which we might call the factor of elasticity. An elasticity factor takes cognizance of variations in constitutional predisposition, *e.g.* an innate sensitiveness to the stimulation of particular instincts or their components. Perhaps the most practical way of expressing this constitutional factor in terms of elasticity is to say that *a normal person must show some capacity for anxiety tolerance*[1]. No matter how stable anyone may appear, the term 'normal' cannot be applied if, during the familiar crises of life, he is swept off his balance into major symptom formation or into prolonged and exaggerated emotional states.

VI. THE ILLUSION OF ABSOLUTE NORMALITY: ITS SIGNIFICANCE.

If, however, exception be taken to these corrections of absolute normality for social adaptation and individual elasticity, we are not yet at the end of our tether. Taking a leaf out of the first book of Euclid, we can preface all discussions of normality with the following postulate: "The introduction of the term absolute in discussions of normality is itself a proof of abnormality." The expedient may suggest sharp practice, yet I maintain that it can be justified on good scientific grounds. Not only does it take due cognizance of the fact that normality standards cannot be relentlessly applied to the man in the street; it throws some light on the functions of one special group of mental mechanisms and illustrates a particular compulsive habit. One of the main functions of the mechanism of rationalization is to help us not to recognize a number of our own personal maladaptations. Rationalization varnishes over the cracks in the egg-shell of character formation. Indeed if we are in a good mood, that is to say, if we are free from anxiety and guilt, we may even go so far as to daub a little of the varnish on our next-door neighbour's carapace. But that is not an invariable practice. On many occasions we derive more satisfaction from innocently inserting a finger nail in the weak places. A second mechanism is really derived from the repression group. We have an unlimited capacity for turning a blind eye to any idiosyncrasies, examination of which would make ourselves uncomfortable. When these two mechanisms run harmoniously together in consciousness, the end result is for all practical purposes a lie. And there is a good deal to be said for the view that the conception of absolute normality is one of those comforting lies with which we beguile our guilt.

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[1] Since writing this I have had an opportunity of reading an address by Ernest Jones on "The Concept of a Normal Mind." Although deprecating any attempt to establish standards of normality, he points out that in the long run the nearest attainable criterion is *fearlessness*, thereby stressing the importance of anxiety systems in psychic development. What I have called "a capacity for anxiety tolerance" represents a clinical modification of this more fundamental standard.

In any case interest in an absolute standard illustrates one of many seemingly normal mental compulsions. Even apparently healthy egoists seldom refrain from maintaining absolute standards for others; but, apart from this, there is a form of endopsychic anxiety which appears to be assuaged by unswerving allegiance to standards of perfection. There is a certain harshness in the quality of these absolutes which is manifested by intolerance of any deviation from their sharply outlined categories. Indeed, one is inclined to believe that, but for the saving humour of common folk, normality might have taken its place alongside other theistic substitutes as an abstract object for adoration. A similar harshness can be detected in many reform movements. And since psychotherapy might be regarded as a variety of reformist activity, it behoves the physician to eliminate as far as possible from his scientific standards any suggestion of private psychological gain. One is tempted to add that the same policy might well be followed by the man in the street: that he should cultivate a greater degree of tolerance of other people's humours and vapours. But that would take us deeply into the motivation of individual and social reform. I have no desire to evade these issues, but my original object was to discuss normality as viewed from the medical psychologist's consulting room; and although I may appear to have trespassed a good deal, I feel entitled to retire within this sanctuary at the end. I do maintain, however, that in return for the privileges and immunities bestowed on him, the medical psychologist is under two definite obligations. He should himself take whatever steps are necessary to eliminate infantile omnipotence and anxiety during business hours at least. And he should never exploit his professional views of life in social relations.

SYNAESTHETIC FACTORS IN JUDGING THE VOICE.

BY MADELINE KERR AND T. H. PEAR.

(*From the Psychological Laboratory, University of Manchester.*)

IN an earlier article in this *Journal* (1) we described a method, apparently new in psychological study, of evoking imagery. Crude dramatic situations were presented, in darkness, by means of electrically reproduced gramophone records. The experimenters hoped that this might provoke imagery more intensely and more frequently than the conventional method of questioning. Furthermore it was expected that the tendency, natural in many listeners, to give the voices a visual interpretation, would excite visual imagery.

These expectations were fulfilled. There was considerable success in evoking synaesthesia. This success, greater than that obtained by question and answer, suggested a further special attempt to increase the frequency of synaesthetic responses.

It is well known that any attempt to describe in words an unusual voice quality is likely to enforce the employment of terms chosen from the vocabulary of senses other than sight. Often the 'borrowed' terms, *e.g.* hard, luscious, purple, velvety (2), may form part of the vocabulary of ordinary speech. This, however, does not fully explain the apparently unique suitability of certain words. To call the process one of simile or metaphor is merely to re-label the problem and to lose it by relegating it to another subject of study. Even if a word is 'only' metaphorical, there is often a wedding of sound with sense. As Wolfgang Köhler points out (3), the nineteenth century over-emphasized the distinction between the various senses.

The use of simile or metaphor may perhaps be regarded as the entrance door to synaesthesia. At the other extreme a person's synaesthetic experiences (*e.g.* of colour) are not only original but take the form of images so disturbingly intense that they may be eidetic (4). Many different types of connection between sensations, images and meanings are confused under the word 'synaesthesia'—a fact evident to the reader of articles upon musical 'coloured hearing.' In English writings few distinctions have been made between different kinds of synaesthesia[1]. Excellent treatments in German are those of Anschütz (6) and Wellek (7).

APPARATUS.

These records were played on a large Columbia gramophone with Marconi electrical pick-up and Marconi dynamic loud-speaker.

MATERIAL.

Of the voices presented by the gramophone, one was speaking and two were singing. All were extreme examples of certain types, presented with little ambiguity or subtlety. They were:

(A) Clergyman's voice speaking, Leonard Henry in "The Village Concert" (H.M.V. No. C 1782).

(B) Greta Keller singing (with orchestra) "Ten Cents a Dance" (Regal No. M. R. 319).

(C) Gracie Fields singing (with orchestra) "Nowt about Owt" (H.M.V. No. B 3291).

METHOD OF EVOKING SYNAESTHESIA.

In record A, a clergyman is opening a village concert. In the part of the record played he makes no actual statement concerning his profession.

Record B is a cynical presentment, both verbally and musically, of the outlook of a tired woman professional partner in a cheap dance hall. The words and the accompaniment make it easy to 'place' this singer. She has a low, husky voice.

In record C, a farm girl describes her courting in broad, and correct, Lancashire dialect.

None of these records, it will be seen, show impeccable taste. They were chosen for this reason.

The hearers were 54 university graduates, of both sexes, taking a diploma course in education. The excerpts were given in the above order. After each had been played, the listeners answered a list of questions containing, among others, the following:

(1) Do you get any imagery which, while seeming to you to be quite important and relevant, might seem to others to be absurd and irrelevant; *e.g.* do you see any of the voices as colours, or feel them as having a particular texture?

(2) Would you apply any of the following adjectives to the voices you have just heard? Write them (*i.e.* the adjectives) down:

woolly, harsh, grating, husky, shrill, clear, muzzy, thick, treacly, silvery, mellow, flat, sugary, velvety, smooth, superficial, hard, soft, streaky, stratified, odorous (state odour), tasting of something (state what), polished, brilliant, sparkling, silky, coloured (state colour or colours).

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[1] An exception is to be found in P. E. Vernon's "Synaesthesia in Music" (5).

RESULTS.

To question 1 were given 19 synaesthetic answers or 35·1 per cent. Some listeners, who did not report synaesthesias to question 1, did so to question 2. This addition brought the total synaesthetic answers (reckoning none twice over) up to 29, *i.e.* 53·7 per cent. Where colours were given as answers to both questions they were found to be the same.

The answers to question 1 are given in full:

(A) *Leonard Henry:* "Image of a bar of steel, dull surface, bent"; "It is a voice with prickles, coming through something soft"; "I get a picture of the voice running out of a machine, like flat felt"; "Imaged as very hard; extremely so"; "Dull grey"; "Reminds me of filing or sandpapering"; "Flinty"; "Blue"; "Dark grey"; "Green"; "Reminds me of acid"; "White grey jelly as sound of voice, white for high notes, darker for low"; "A sandy flat feeling, bare like sandhills"; "Grey voice, rich texture, not too pleasant to touch"; "Feeling of silver and pink."

(B) *Greta Keller:* "Triangular prisms"; "Limelights through dusty air"; "A smoothly undulating flower of soft cement, blue"; "Seems as if she is singing through a stove-pipe"; "Light-green"; "Acid grey voice with frilly edges"; "See the high notes as threads"; "Affects me like a brilliantly polished stone of no particular beauty but that cuts"; "Brown trying to be green, exceedingly poor and threadbare"; "Black and pale pink."

(C) *Gracie Fields:* "A tight thread splitting over a knife-edge, or a knife driven slowly and with much squeaking over a smooth surface"; "Rough but not coloured"; "Rather like a shallow swift brook running over big sharp stones"; "Bright colours"; "An impression of parallel horizontal lines, rather thick. Feel as though I am having a bad tooth drilled or someone has cut my finger nail too short with a rather blunt pair of scissors."

Synaesthesias can be classified as sensed, imaged and thought. Since no individual was questioned in this experiment, there is no means of ascertaining from the results if any synaesthesia was actually *sensed.*

CONCLUSIONS.

The percentage of instances in which synaesthesia is reported by normal persons can be raised considerably by presenting emotionally accentuated, crude 'situations.' This is accounted for by two factors (*a*) actual perception instead of imaging, (*b*) a more intense affective tone in the experience.

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(1) Kerr, M. and Pear, T. H. (1931). "Unseen drama and imagery, some experimental observations." *Brit. J. Psychol.* xxii, 43.

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(6) Anschütz, G. (1929). *Das Farbe-Ton Problem in psychischen Gesamtbereich*. Halle.

(7) Wellek, A. (1931). "Zur Geschichte und Kritik der Synaesthesie-Forschung." *Arch. für d. ges. Psych.* lxxix, 325–77.

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THE MATHEMATICAL AND EXPERIMENTAL EVIDENCE FOR THE EXISTENCE OF A CENTRAL INTELLECTIVE FACTOR (g)[1].

By WILLIAM BROWN.

(*Oxford and London.*)

If a number of sufficiently dissimilar mental tests of intellective ability be applied to a group of individuals and correlation coefficients calculated, it is found that these correlation coefficients are related to one another in such a way that for any four (or *tetrad*) of them the following relation holds good, within the limits of random sampling, viz.

$$r_{ap}r_{bq} - r_{aq}r_{bp} = 0 \qquad \ldots\ldots(1),$$

and similarly with other arrangements of these four tests. We owe both the discovery of fact and the devising of the tetrad criterion to Prof. C. Spearman.

The inference drawn from this is that the abilities measured by the mental tests are divisible into two factors each, the one being common to all (the general factor, g), while the other is in each case specific and independent (s).

In Prof. Spearman's own words: "Whenever the tetrad equation holds throughout any table of correlations, and *only* when it does so, then every individual measurement of every ability (or of any other variable that enters into the table) can be divided into two independent parts which possess the following momentous properties. The one part has been called the 'general factor' and denoted by the letter 'g'; it is so named because, although varying freely from individual to individual, it remains the same for any one individual in respect of all the correlated abilities. The second part has been called the 'specific factor,' and denoted by the letter 's.' It not only varies from individual to individual, but even for any one individual from each ability to another[2]."

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[1] Communicated to Section J (Psychology) of the British Association for the Advancement of Science, London, Sept. 25th, 1931,—and, with certain additions, to the Tenth International Congress of Psychology at Copenhagen, Aug. 24th, 1932.

[2] C. Spearman, *The Abilities of Man*. London: Macmillan & Co. Ltd. 1927, pp. 74, 75.

The relationship is expressed by the following equation:

$$m_{ax} = r_{ag} \cdot g_x + r_{as_a} \cdot s_{ax} \qquad \ldots\ldots(2),$$

where m_{ax} = the measurement obtained for any individual x in the variable a, g_x = the individual's amount of g, the factor common to all the variables, and s_{ax} = the individual's amount of s_a, the factor specific to the variable a.

The method of applying the tetrad criterion is to draw up a frequency distribution of all the possible tetrad differences derivable from the table of correlation coefficients (there being $3\,{}^nC_4$ positive tetrad differences and an equal number of negative ones, where n is the number of mental tests correlated with one another) and to compare its standard deviation with the 'theoretical' standard deviation of a purely chance distribution of such tetrad differences. A formula for the latter has been calculated by Spearman and Holzinger[1], viz.

$$\bar{\sigma}_t{}^2 = \frac{4}{N}\left\{\bar{r}^2(1-\bar{r})^2 + \left[1 - 3\bar{r}\frac{(n-4)}{(n-2)} + 2\bar{r}^2\frac{(n-6)}{(n-2)}\right]s^2\right\} \quad \ldots(3),$$

where N = number of cases, n = number of tests, r = mean of correlation coefficients, s = standard deviation of correlation coefficients, and $\bar{\sigma}_t$ = the average value of the standard deviation of the tetrad differences.

In applying this formula, σ_t is generally multiplied by 0·67449 to give a 'conventional' P.E., but since the frequency curve of tetrads is not, and cannot be, an exact probability curve (because the correlation coefficients, and therefore the tetrads, are not uncorrelated with one another), although it approximates to one, there is little to be gained by this procedure.

As the formula for the probable error of tetrads allows for the effects of 'attenuation' upon correlation coefficients, so that these coefficients need not be corrected for observational errors, the 'tetrad criterion' escapes criticism of mine[2] which was, as I still contend, rightly directed, in certain cases, towards the 'correction formulae' which Prof. Spearman devised in 1906 and 1910 to adjust for such errors. It also takes the place of the criterion of 'intercolumnar correlation,' or correlation between columns of correlations in a table of correlation coefficients, upon which Prof. Spearman previously relied in proving his theory. According to this criterion, the average intercolumnar correlation should approximate to unity if the Theory of Two Factors holds good[3]. But the correlation coefficients had first to be 'corrected' for errors of observation (by formulae whose general or universal applicability I dispute), and even the criterion was only applicable to those coefficients which reached a certain 'correctional standard,' and thus did not admit of application to the whole table of correlation coefficients, as the tetrad criterion does.

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[1] Spearman, C. and Holzinger, K., "The Average Value for the Probable Error of Tetrad Differences." *Brit. Journ. Psychol.* (1930), XX, 370.

[2] *Essentials of Mental Measurement.* Cambridge: The University Press, 1st edition, 1911, pp. 83–85. *Brit. Journ. Psychol.* (1913), VI, 223.

[3] This is the famous 'hierarchical order' of correlation coefficients.

In my own research work with mental tests in 1909, 1910 and 1913, I did not feel justified in considering my results as confirmatory of Prof. Spearman's theory, because of the above-mentioned difficulties. But on the other hand I never contended that my results *disproved* his theory. In the *Essentials of Mental Measurement*, 1st edition, 1911, I wrote: "A definite solution of the question of the existence or non-existence of one central mental ability is yet to be sought. It can only be obtained by the use of much larger random samples than those hitherto employed, since the probable errors must be small compared with the coefficients, if precise inferences are to be drawn from the latter, and in the case of small samples this condition is satisfied only for *large* correlation coefficients, which when obtained are often merely the result of selecting tests which measure closely similar mental abilities. In all results hitherto quoted in support of ultimate identity of general intelligence and general sensory discrimination, the correlations contributed by the latter are so small compared with their P.E.'s that nothing definite can be inferred from them" (p. 120). My verdict at that date had to be 'Non-proven,' but certainly not 'Disproved.'

Nevertheless, in the interests of history and of scientific completeness, it seems worth while to work over some of those earlier results of mine with the aid of the tetrad criterion. I have done this with (I) a group of 66 boys, aged 11–12 years, of an Elementary School (*Essentials of Mental Measurement*, 1st edition, p. 114), (II) a group of 40 boys, aged 11–12 years, of a Higher Grade School (*ibid.* p. 116)[1], and (III) a group of 83 boys, aged 14–16 years, of a Public School (St Paul's School), examined in mathematics only[2]. In the first two groups I have pooled correlations between too-closely related abilities, in conformity with Prof. Spearman's criticism, and in the third group I have refrained from 'partialling out' for difference of form (the boys were drawn from five different forms) as this difference itself was some measure of difference of mathematical ability.

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[1] Both in "Some experimental results in the correlation of mental abilities," *Brit. Journ. Psychol.* (1910), III, 296.

[2] "An Objective Study of Mathematical Intelligence." *Biometrika* (1910), VII, 352. The table of correlation coefficients, uncorrected for difference of form, is given in *Mind and Personality*, University of London Press, Ltd., 1926, p. 123.

In Group I, with 8 tests, the total number of positive tetrad differences is 210. The observed median tetrad difference is 0·0208, and the estimated value is 0·024, which may be compared with the 'theoretical' P.E. value, calculated by the Spearman and Holzinger formula, 0·0353. Moreover, $\sigma_t = 0{\cdot}0456$, which gives another value of the conventional P.E., viz. $0{\cdot}67449\,\sigma_t = 0{\cdot}03076$, which is also below the 'theoretical' value. These results, so far as they go, are all in favour of Spearman's two-factor theory. The distribution of tetrads is markedly leptokurtic, and $\beta_2 = 4{\cdot}275$, as compared with $\beta_2 = 3$ for a Probability Curve.

In Group II, with 8 tests and therefore 210 positive tetrads, the median tetrad difference is 0·0528. The 'theoretical' P.E. (S. and H.) is 0·0495, which shows a difference of 0·0033 on the wrong side. But $\sigma_t = 0{\cdot}069$, giving another value of the conventional P.E. 0·0465, which is in conformity with the two-factor theory.

The distribution of tetrads is markedly platykurtic. $\beta_2 = 2{\cdot}358$.

In Group III, with 9 subsidiary mathematical abilities intercorrelated, and therefore 378 positive tetrads, the results are:

Median tetrad = 0·0761, 'Theoretical' P.E. (S. and H.) = 0·0379,

Mean tetrad = 0·1201,

$\sigma_t = 0{\cdot}1612$, giving 'Conventional' P.E. = 0·1087,

$$\mu_2\ (i.e.\ \sigma_t^2) = 0{\cdot}02601,\ \mu_4 = 0{\cdot}002637.\ \therefore\ \beta_2 = \frac{\mu_4}{\mu_2^2} = 3{\cdot}9041,$$

i.e. > 3 (a leptokurtic curve).

Although this group furnishes a smooth frequency curve of tetrad differences, the results give little support to any theory of a central *mathematical* factor. No such theory has been put forward by Prof. Spearman himself, and I have only introduced this group as a good illustration of the working out of the tetrad criterion on fairly adequate statistical material[1]. Whether the excess of σ_t over the corresponding Spearman-Holzinger value is statistically significant could only be decided with precision by determining the standard deviation of σ_t. This involves the use of complicated formulae devised by Prof. Karl Pearson[2].

But the results in Groups I and II, as far as numbers allow, do support the existence of a central intellective factor (g), and, when taken in relation with the large body of similar evidence accumulated during the last twenty years by Prof. Spearman and his students, help to give solid basis to that theory. A mathematically satisfactory proof of the theory would involve much larger numbers, both of individuals tested and also of non-overlapping tests of intellective ability to be used upon them. In a critical article by Prof. Karl Pearson[1] on this subject the following paragraph appears: "We suggest that some 12 to 15 abilities (66 to 105 correlations, 1485 to 4095 tetrads), the abilities being settled by psychologists *a priori* to avoid 'overlaps,' are essential to a satisfactory test, the observations to be made on a homogeneous population of several hundreds. Short series involve such large probable errors that a mere statement that theory and observation are in accordance within the limits of the probable errors can carry no conviction with it."

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[1] My thanks are due to Mr R. J. Bartlett, M.Sc., for calculating tetrad differences and certain constants in this research. I have myself re-calculated all the results, apart from the tetrad differences.

[2] Applying these formulae, I find P.E. of $\sigma_t = {\cdot}001175$, and therefore P.E. of P.E. = ·0007928. Hence the evidence is against the existence of a central *mathematical* factor.

I have organized a research along these lines, with the kind help of Prof. Spearman and Dr W. Stephenson of University College, London. Dr Stephenson has devised a series of 20 tests of apparently non-overlapping intellective ability (selected not exactly *a priori* but after much careful preliminary trial), which received the approval of Prof. Spearman, and has applied them for me to 300 boys, aged 10½–11 years, drawn from 12 Elementary Schools of the L.C.C., forming a homogeneous 'random sample' of adequate size for statistical purposes. The total number of positive tetrad differences is 14,535. It has since been found necessary to reject one of the tests and one of the correlation coefficients. There remain 11,356 positive tetrads which form a smooth frequency curve, the mathematical properties of which I am now working out. I have found that the best-fitting frequency curve is a Type II A Pearson curve, with equation

$$y = 1412\left(1 - \frac{x^2}{1188}\right)^{13\cdot669}. \text{ [Unit of grouping} = \cdot005.]$$

The curve is platykurtic, with $\beta_2 = 2\cdot81446$. The standard deviation, σ_t, $= 0\cdot031289 \pm \cdot002586$. If we compare this with the 'theoretical' Spearman-Holzinger value, $\bar{\sigma}_t = 0\cdot02827$, we find an excess of 0·003019, being 1·167 times the probable error. This means that the odds are 71 to 54 (about 4 to 3) against such a deviation—a good correspondence of observation with theory. But only after much further psychological and mathematical analysis of the enormous mass of data can a final conclusion be drawn. The material satisfies the most stringent demands of statistical theory and will furnish a precise and definite solution of the problem of a Central Intellective Factor. A joint paper by Dr Stephenson and myself on the whole research is now in preparation and will appear shortly.

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[1] Karl Pearson and Margaret Moul, "The mathematics of intelligence, I. The sampling errors in the theory of a generalised factor." *Biometrika*, XIX, 261.

APPENDIX

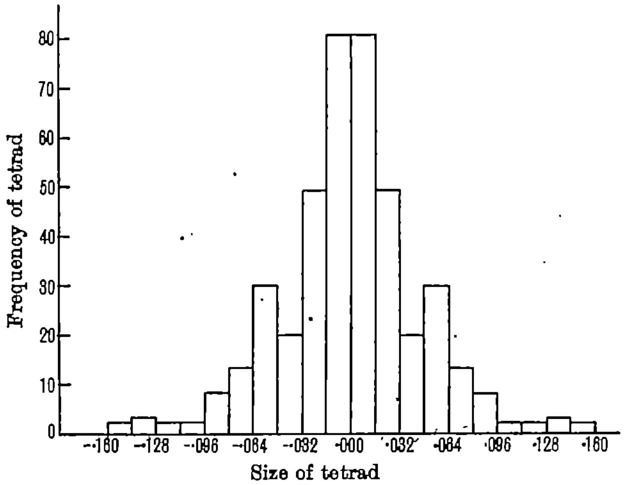
Group I. 66 *boys, aged* 11–12 *years. Elementary School.*

Correlation Square.

	1	2	3	4	5	6	7	8
1. Combination	—	0·52	0·52	0·39	0·46	0·13	0·00	0·15
2. Memory, poetry	0·52	—	0·49	0·39	0·27	0·12	0·05	0·13
3. Memory, mechanical	0·52	0·49	—	0·29	0·34	0·14	0·12	0·10
4. Addition	0·39	0·39	0·29	—	0·41	0·12	0·03	0·20
5. Letters (ER+ANOS)	0·46	0·27	0·34	0·41	—	0·37	0·10	0·00
6. Motor Ability	0·13	0·12	0·14	0·12	0·37	—	0·04	0·00
7. Illusion (M.-L.)	0·00	0·05	0·12	0·03	0·10	0·04	—	0·16
8. Bisection	0·15	0·13	0·10	0·20	0·00	0·00	0·16	—

No. of +ve tetrad differences of form $r_{13}r_{24}-r_{14}r_{23}=3\times{}^8C_4=210$.

Symmetrical Distribution of Tetrad Differences.



Distribution of Positive Tetrads.

Range of tetrad	0·000–0·016	0·016–0·032	0·032–0·048	0·048–0·064	0·064–0·080	0·080–0·096	0·096–0·112	0·112–0·128	0·128–0·144	0·144–0·160	Total
Frequency	81	49	20	30	13	8	2	2	3	2	210

Observed median tetrad difference = 0·0208.
Estimated ,, ,, ,, = 0·024.
Observed σ of tetrad differences = 0·0456.
∴ Conventional P.E. = 0·67449σ = 0·03076.
'Theoretical' P.E. (Spearman and Holzinger) = 0·0353.
$\mu_2=0{\cdot}00207846$,

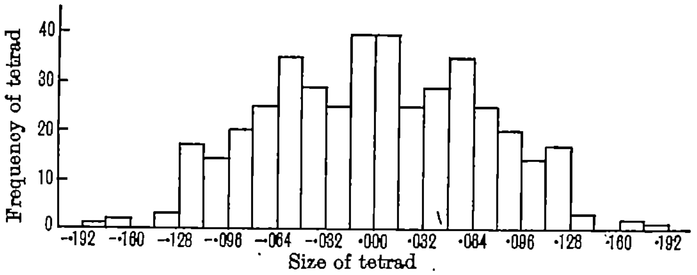
$\mu_4=0{\cdot}000018469$, $\therefore\ \beta_2=\dfrac{\mu_4}{\mu_2^2}=4{\cdot}275$ (a leptokurtic curve).

GROUP II. 40 *boys, aged* 11–12 *years. Higher Grade School.*

Correlation Square.

	1	2	3	4	5	6	7	8
1. Intelligence (S. Marks + Gen. Intell.)	—	0·585	0·645	0·465	0·26	0·23	0·445	0·275
2. Memory, poetry	0·585	—	0·44	0·44	0·00	0·185	0·38	0·19
3. Combination	0·645	0·44	—	0·46	0·32	0·05	0·28	0·28
4. Drawing	0·465	0·44	0·46	—	0·14	0·15	0·39	0·00
5. Addition (speed)	0·26	0·00	0·32	0·14	—	0·275	0·00	0·20
6. Letters (ER + ANOS)	0·23	0·185	0·05	0·15	0·275	—	0·00	0·125
7. Memory, mechanical	0·445	0·38	0·28	0·39	0·00	0·00	—	0·00
8. Motor Ability (all letters)	0·275	0·19	0·28	0·00	0·20	0·125	0·00	—

Symmetrical Distribution of Tetrad Differences.



Median tetrad difference = 0·0528.
Observed σ of tetrad differences = 0·069.
∴ Conventional P.E. = 0·0465.
'Theoretical' P.E. (S. and H.) = 0·0495.
$\mu_2 = 0{\cdot}004755$, $\mu_4 = 0{\cdot}000053308$,
∴ $\beta_2 = 2{\cdot}358$ (a platykurtic curve).

Distribution of Positive Tetrads.

Range of tetrad	0·000–0·016	0·016–0·032	0·032–0·048	0·048–0·064	0·064–0·080	0·080–0·096	0·096–0·112	0·112–0·128	0·128–0·144	0·144–0·160	0·160–0·176	0·176–0·192	Total
Frequency	39	25	29	35	25	20	14	17	3	0	2	1	210

GROUP III. 83 *Public School Boys, aged* 14–16 *years.*
Mathematical Examination, 1910.

Correlation Square.

	C	H	E	I	G	A	F	D	B
C	—	0·57	0·47	0·55	0·59	0·78	0·51	0·81	0·60
H	0·57	—	0·69	0·92	0·53	0·61	0·55	0·40	0·26
E	0·47	0·69	—	0·57	0·76	0·44	0·82	0·46	0·23
I	0·55	0·92	0·57	—	0·49	0·47	0·49	0·43	0·22
G	0·59	0·53	0·76	0·49	—	0·41	0·64	0·44	0·26
A	0·78	0·61	0·44	0·47	0·41	—	0·46	0·65	0·49
F	0·51	0·55	0·82	0·49	0·64	0·46	—	0·49	0·28
D	0·81	0·40	0·46	0·43	0·44	0·65	0·49	—	0·37
B	0·60	0·26	0·23	0·22	0·26	0·49	0·28	0·37	—

Geometry.

A. Memory of Definitions and General Principles (*e.g.* principle of superposition).
B. Memory of constructions.
C. Memory of preceding propositions and power of applying them.
D. Recognition of necessity of generality in proof, and power of recognizing general relations in a particular case.

Arithmetic.

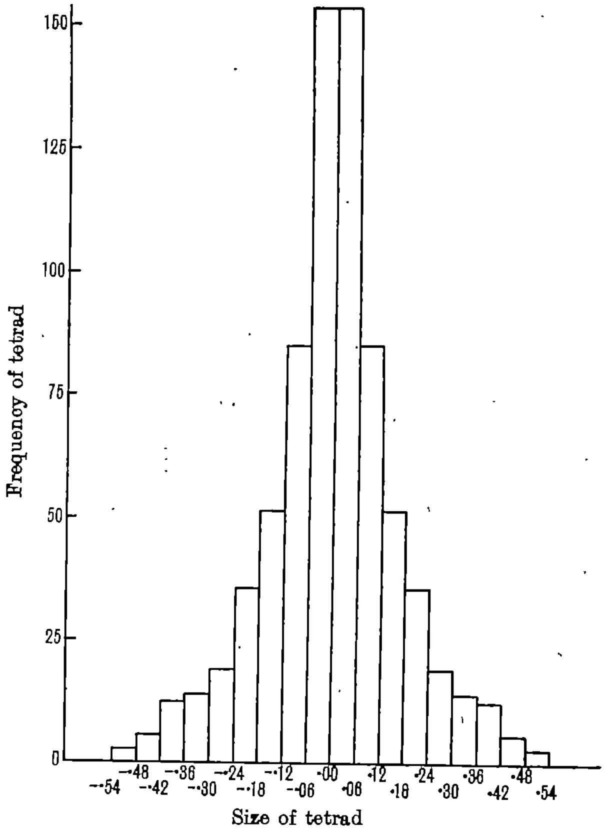
E. Accuracy.
F. General memory of rules and power of applying them.
G. Power of doing sums in percentage and proportion.

Algebra.

H. Accuracy.
I. General memory of rules and power of applying them.

No. of +ve tetrad differences of form $r_{13}r_{24}-r_{14}r_{23}=3\times{}^9C_4=378$.

Symmetrical Distribution of Tetrad Differences.



Distribution of Positive Tetrads.

Range of tetrad	0·00–0·06	0·06–0·12	0·12–0·18	0·18–0·24	0·24–0·30	0·30–0·36	0·36–0·42	0·42–0·48	0·48–0·54	Total
Frequency	153	85	52	36	19	13	12	6	2	378

The constants of this distribution are:

Median tetrad = 0·0761. Standard deviation, σ_t, = 0·161276.

Mean tetrad = 0·1201. $0{\cdot}67449\sigma_t = 0{\cdot}10878$.

'Theoretical' P.E. (S. and H.) = 0·0379.

$\sigma_t^2 = \mu_2 = 0{\cdot}02601$, $\mu_4 = 0{\cdot}002637$.

$$\therefore\ \beta_2 = \frac{\mu_4}{\mu_2^{\,2}} = 3{\cdot}9041 \text{ (a leptokurtic curve).}$$

The best-fitting curves to the distributions are Type II A (platykurtic) and Type II B (leptokurtic) Pearson curves. The actual equations to three curves are:

for Group I,
$$y = \frac{65{\cdot}11}{\left(1 + \dfrac{x^2}{54{\cdot}445}\right)^{4{\cdot}853}};$$

for Group II,
$$y = 34{\cdot}754\left(1 - \frac{x^2}{136{\cdot}43}\right)^{2{\cdot}174};$$

for Group III,
$$y = \frac{121{\cdot}63}{\left(1 + \dfrac{x^2}{62{\cdot}3976}\right)^{5{\cdot}818}}.$$

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VISUAL IMAGERY AND RETENTION.

By HENRY BOWERS.

(*From the Normal School, Ottawa.*)

I. *Introduction* (p. 180).
II. *The problems* (pp. 180–1).
III. *Outline of method and conclusions* (pp. 181–2).
IV. *General plan of experiment* (p. 182).
V. *Results of problems* (pp. 182–92).
  (1) *Problem* 1 (pp. 182–4).
  (2) *Problem* 2 (pp. 184–9).
    (*a*) *Experiment in which words were used* (pp. 184–7).
    (*b*) *Experiment in which letter-groups were used* (pp. 187–9).
  (3) *Problem* 3 (p. 189).
  (4) *Problem* 4 (pp. 189–92).
VI. *Discussion of the results obtained in connection with problems* 1, 2 *and* 4 (pp. 192–4).
VII. *Conclusions* (pp. 194–5).
References (p. 195).

I. Introduction.

It has been shown (1) for a certain list of forty-five words that a small but significant correlation exists between the mean concrete visual imagery claimed for each word and the number of times it was remembered. The number of subjects used was small, which of course involved considerable instability in both these variables, and more especially in the latter. On this account it was deemed advisable to carry out another experiment in which the number of subjects was substantially increased. At the same time an attempt was made to find to what extent visual verbal imagery and retention of these words varied concomitantly.

II. The problems.

1. To determine the degree of correspondence between the mean claimed concrete visual imagery for words in a given list and the number of times these words were recalled.

2. To determine the degree of correspondence between the mean claimed visual verbal imagery for words or letter-combinations and the number of times those words or letter-groups were recalled.

3. To find if significant difference in this correspondence exists between words in the first and last thirds of the reproduction lists.

4. To find if groups of subjects claiming high and low concrete or visual verbal imagery differ in respect of the number of words or letter-combinations recalled.

III. OUTLINE OF METHOD AND CONCLUSIONS.

A list of 139 monosyllabic words, the concomitant imagery of which was in the main likely to be visual, was composed by making three selections of the same forty-five words. The words in each selection were in a different chance order. Two 'dummy' words were placed at the beginning and two at the end in order to counteract the influence of primacy and recency.

This list was read to 128 subjects at the rate of one word per second. In the case of another group of 122 subjects the presentation was visual. Immediate reproduction of the words in any order was required.

The subjects in the former group rated on a six-point scale the concrete visual imagery aroused by each word, those in the latter group rated the visual verbal imagery on the same scale.

Thus, for each word were known the 'mean image,' visual or concrete, and the number of times that word was remembered. Coefficients of correlation indicative of the degree of stability of (*a*) the mean claimed imagery, and (*b*) the number of times each word was remembered were calculated. Pearson *r* between (*a*) and (*b*) was also found. Also, groups of subjects claiming high and low imagery were compared in regard to the total number of words recalled.

Seventeen letter-groups of three consonants each were prepared. These were presented visually to 251 subjects and immediate reproduction was required. The mean claimed visual verbal images were found. A statistical treatment similar to that indicated above was made.

It was found that:

1. A significant positive correlation exists between the number of times these particular words were remembered and the concrete visual imagery claimed for them. This confirms the previous finding.

2. (*a*) In the case of these forty-five words a smaller but still significant correlation exists between the number of times they were remembered and the mean claimed visual verbal imagery.

(*b*) For the nineteen letter-combinations the coefficient was somewhat larger.

3. The correspondence between memory and imagery is more marked

in the first third of the reproduction list than in the last third for concrete imagery and for visual verbal imagery when letter-groups were used. There is slight evidence of reversal of this finding for visual verbal imagery when words were employed.

4. (*a*) There is some evidence that groups of subjects claiming high concrete imagery recall more words than groups whose claimed imagery is low.

(*b*) No difference could be found between groups claiming high and low visual verbal imagery in the matter of recall.

5. It is confirmed that "just as there appears to be a relatively constant order of precedence in words in respect of the mean imagery attached to them by a given group, so there would seem also to be an order of memorial precedence"(1).

6. The order of 'memorial precedence' depends on the mode of presentation.

IV. GENERAL PLAN OF EXPERIMENT.

In thea rticle "Memory and mental imagery" (1), data were reported concerning the influence upon correlation between imagery and recall, of variations in procedure affecting (*a*) interval between presentation and reproduction, (*b*) time required in presenting each word. No difference could be detected between the correspondence of imagery and recall when reproduction was required at once or at an interval of two minutes. The matter of interval before reproduction will not be considered in this paper. Since no consistent variation was produced in the coefficient by varying the time of presenting each word this factor was kept constant in the present experiment. Also, an attempt to make a partial elimination of association between the words was described. This was without apparent effect and was not repeated.

In the previous experiment no account was taken of the possible effect of visual verbal imagery. Data connected with this factor will be reported.

V. RESULTS OF PROBLEMS.

(1) *Problem* 1.

The forty-five monosyllabic words used in the former experiment were again employed. It might be objected that several words in this list could hardly be described as potential evokers of concrete visual imagery. However, it will be noted that these words are among the lowest in mean claimed imagery. The first figure after each word is the mean claimed concrete visual image (128 subjects), the second is the mean claimed visual verbal image (117 subjects). A six-point scale ((2) p. 217), 0, 1, 2, 3, 4, 5, was used in each case.

beard (4·0; 3·1), boat (4·0; 3·3), clan (1·9; 3·2), dime (4·3; 3·1), elf (2·4; 3·3), fiend (2·1; 2·9), ghost (2·8; 2·9), grass (4·4; 3·2), hat (4·5; 3·7), hen (4·1; 3·6), jug (4·1; 3·2), path (3·9; 3·0), pew (3·7; 3·1), phrase (2·4; 2·4), plank (3·7; 2·8), porch (4·0; 3·0), pouch (2·7; 3·0), prince (2·6; 3·1), purse (4·0; 3·1), quill (2·3; 3·0), rib (2·5; 3·5), rogue (1·9; 2·9), rug (3.9; 3·5), rust (3·3; 3·3), scar (3·7; 3·1), shrub (3·6; 2·7), sky (4·3; 3·3), slum (2·0; 3·2), snake (3·8; 2·8), swan (2·9; 3·2), tent (4·0; 3·5), thief (2·0; 2·8), toad (3·4; 3·1), tongs (3·4; 2·8), tweed (2·8; 2·9), twin (3·2; 3·2), valve (2·3; 2·9), van (3·2; 3·5), verb (2·9; 3·2), wad (2·2; 3·1), wand (3·0; 3·0), wart (3·6; 2·9), weed (3·8; 3·2), wig (3·2; 3·3), wisp (2·0; 3·0).

From these words a list of 135 was made (1). Four 'dummy' words only were added. The list of 139 words was read to a group of 128 teachers in training, at the rate of one word per second. It was requested that tendencies to speaking movements be inhibited as far as possible. Immediately after the reading the subjects wrote the words in any order. Some weeks later the concrete visual image evoked by each of the forty-five words was rated. None of the subjects had had previous experience in this type of introspection.

The group of 128 subjects was divided at random into two equal groups. For each of these groups the mean concrete imagery claimed for each *word* was found, and also the total number of times each *word* was recalled. Pearson r for the former two sets of figures was $+ 0{\cdot}95 \pm 0{\cdot}01$ ($N = 45$; 64 and 64Ss), and for the latter $+ 0{\cdot}74 \pm 0{\cdot}05$ ($N = 45$; 64 and 64Ss). These two coefficients enable us to fix upper and lower limits to the coefficient of correlation between the mean claimed imagery for each word and the number of times it was recalled by the whole group of 128 subjects. The uncorrected coefficient between these figures was $+ 0{\cdot}46 \pm 0{\cdot}08$ ($N = 45$; 128 and 128Ss). It is clear that the coefficients 0·95 and 0·74 would be increased if the whole group of 128 subjects were taken instead of the half group of 64, but not to the extent predicted by the Spearman-Brown formula, viz. to 0·98 and 0·85 respectively. The value of each of these coefficients when the whole group is taken into account must lie between 0·95 and 0·98 and between 0·74 and 0·85. Substituting the extreme values in the well-known formula for correcting for attenuation we find that the coefficient + 0·46 is raised to a value between the limits + 0·51 and + 0·55. However, as a matter of conservatism we shall use the uncorrected coefficient. Later it will be shown that this figure is reduced to + 0·42 when the influence of visual verbal imagery is 'partialled out.'

Another approach to this problem of imagery and retention was made by comparing three groups of words, of high, medium and low claimed imagery, in respect of the average number of times each word was recalled by the 128 subjects. The eleven words of highest, eleven of medium, and the eleven of lowest claimed imagery were chosen. Arithmetic means and medians are reported in Table I.

Table I. *Comparison of groups of words in respect of average concrete imagery claimed and number of times recalled.*

Position of group in range of imagery	Average claimed imagery		Average number of times recalled	
	Arithmetic Mean	Median	Arithmetic mean	Median
Highest 11	4·15	4·10	51·5	52·0
Middle 11	3·10	3·20	36·2	30·0
Lowest 11	2·15	2·10	28·6	25·0

These averages are in agreement with the positive correlation reported above. It is worthy of note that words of the highest claimed imagery are markedly in advance of those of medium and lowest imagery in the matter of recall. The following analysis shows the extent to which overlapping took place in the number of times individual words were recalled.

Highest 11: One word below the median of the lowest 11.

Middle 11: Two words above the median of the highest 11; three words below the median of the lowest 11.

Lowest 11: One word above the median of the highest 11; two words above the median of the middle 11.

(2) *Problem* 2.

(*a*) *Experiment in which words were used.*

In this experiment the same 45 words were used. Each of the 122 subjects[1] was given two strips of paper on which the 139 words were typed in single column arrangement. At the beginning the words were covered with a sheet of paper. A word was uncovered each second when a sharp tap was given. It should be made clear that although one new word was displayed every second those already looked at were still in full view. It was requested that speaking movements be inhibited. Immediate reproduction of the words in any order was required. Some weeks later practice was given in rating the visual verbal images aroused by letter-groups. It was clear that after-images were quite prevalent. There was also the strong probability of the presence of memory after-images. The subjects were requested to allow these images to fade out before assigning a rating. Each of the 45 visual verbal images was rated by 117 subjects on the six-point scale previously employed. A random division of the 117 subjects was made into two groups one containing 59, the other 58 subjects. The 122 subjects were divided into two equal groups. As in the case of the concrete visual imagery coefficients of correlation were calculated between the two sets of mean claimed images and also between the two sets of numbers obtained from the reproduction[1]. Pearson r's were + 0·84 ± 0·03 ($N = 45$; 58 and 59 *Ss*) and + 0·88 ± 0·02 ($N = 45$; 61 and 61 *Ss*), respectively. The coefficient of correlation between mean claimed visual verbal imagery for each word and the number of times it was recalled was + 0·36 ± 0·09 ($N = 45$; 117 and 122 *Ss*). Corrected for attenuation as before this coefficient lies between + 0·39 and + 0·42.

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[1] These subjects were not used in the previous experiment.

Table II. *Comparison of words of same mean visual verbal imagery but of different length in respect of retention.* (*Number of subjects*, 117.)

Words	Mean visual verbal imagery	Number of times long word was remembered	Number of times short word was remembered
Prince—Wad	3·1	37	27
Beard—Pew	3·1	35	55
Grass—Jug	3·2	80	49
Tent—Rib	3·5	50	31
Rust—Elf	3·3	33	92
Boat—Sky	3·3	38	74
Purse—Dime	3·1	40	34
Porch—Path	3·0	45	11
Pouch—Wand	3·0	51	15
Quill—Wisp	3·0	62	31
Fiend—Wart	2·9	65	56
	Totals	536	475

On reference to the figures given for the mean visual verbal images of the forty-five words it is apparent that the images of the shorter words are rated more highly than those of the longer ones. On this account there arises the possibility that the positive correlation (+ 0·36) reported above may be due to ease in remembering short rather than long words. To throw some light on this problem words of the same mean claimed visual verbal imagery were paired. The words in each pair were of unequal length. Table II contains the relevant data.

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[1] Pearson r between the number of times the words were recalled when presented auditorily and the number when presented visually was + 0·39 ± 0·08 ($N = 45$; 128 and 122 *Ss*). This indicates that the memorial order depends on the mode of presentation.

If there is any definite tendency here it is that the longer rather than the shorter words are favoured in retention.

Another method of dealing with this question is to compare words of the same number of letters but of different imagery. This is done in Table III.

Table III. *Comparison of words of same length but of different mean visual verbal imagery in respect of retention.* (*Number of subjects*, 117.)

Word	Mean visual verbal imagery	Number of times word was remembered		Word	Mean visual verbal imagery	Number of times word was remembered	
Hat	3·7	31	115	Pew	3·1	55	82
Hen	3·6	84		Wad	3·1	27	
Tent	3·5	50	121	Wand	3·0	15	102
Rust	3·3	33		Wisp	3·0	31	
Boat	3·3	38		Wart	2·9	56	
Grass	3·2	80	155	Shrub	2·7	21	86
Beard	3·1	35		Tongs	2·8	33	
Purse	3·1	40		Thief	2·8	32	

There is apparently no ground for the supposition that in this particular list of words the positive correlation between verbal imagery and retention is due to the fact that shorter words are easier to remember.

Two other questions arise, one concerning the possible contribution of concrete imagery to the correlation between verbal imagery and recall, the other in connection with the possible influence of verbal imagery upon the relationship between concrete imagery and recall. The coefficient of correlation between mean concrete imagery and mean visual verbal imagery for the 45 words was found to be + 0·34 ± 0·09 ($N = 45$; 128 and 117 *Ss*). This is a surprisingly high figure and might suggest that a certain proportion of subjects was influenced by concrete imagery in rating verbal imagery. There is, however, another and more likely reason, namely, pure coincidence. This is illustrated by considering three words, 'hat,' 'hen,' 'phrase.' It is obvious enough that the first two would on the average arouse relatively clear concrete images and the third, relatively vague. On the other hand one can readily see why an exactly parallel situation should hold in the case of verbal imagery. Elimination of these three words lowers the coefficient of + 0·34[1], referred to above, to the substantially smaller one, + 0·21 ± 0·10 ($N = 42$). Using the partial coefficient technique the figures reported in Table IV were found.

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[1] In calculating the partial coefficients of correlation reported in Table IV this figure was, of course, used.

Table IV. *Coefficients of correlation between imagery and retention.*

Concrete imagery and retention (auditory presentation); verbal imagery constant	+ 0·42.
Verbal imagery and retention (auditory presentation); concrete imagery constant	+ 0·07.
Verbal imagery and retention (visual presentation); concrete imagery constant	+ 0·33.
Concrete imagery and retention (visual presentation); verbal imagery constant	+ 0·02.

It will be noted that the original coefficients are not altered materially.

A comparison of the average retention of groups of words taken from different parts of the imagery range is of some interest. The data are reported in Table V.

Table V. *Comparison of groups of words in respect of average verbal imagery claimed and number of times recalled.*

Position of group in range of imagery	Average claimed imagery		Average number of times each word was recalled	
	Arithmetic mean	Median	Arithmetic mean	Median
Highest 11	3·45	3·50	55·4	50·0
Middle 22	3·10	3·10	39·5	36·5
Lowest 12	2·80	2·85	38·2	33·5

These figures are in general agreement with the coefficients found. As in the case of concrete imagery, the words of highest claimed imagery are considerably in advance of those of medium and lowest imagery in the matter of recall. Again there is overlapping. For instance, of the eleven words of highest verbal imagery three are remembered fewer times than the median of the middle twenty-two. Of the lowest twelve there are four remembered more frequently than the median of the highest eleven, and one other remembered oftener than the median of the middle twenty-two.

(*b*) *Experiment in which letter-groups were used.*

The following seventeen letter-combinations were used as experimental material:

wqg (2·6; 55), cbl (3·8; 83), mgw (3·0; 90), cjv (3·4; 116), qfg (2·7; 62), bnc (3·6; 98), grk (3·0; 121), lvc (3·4; 107), gdn (3·1; 62), nlj (2·9; 49), kwf (3·0; 66), vtj (3·0; 108), lht (3·3; 56), qkm (3·0; 82), tnv (3·1; 66), dqw (2·8; 21), plt (3·5; 78).

After each group are two figures; the first is the mean claimed visual verbal imagery for 251 subjects, the second, the total number of times the group was recalled by all these subjects. The procedure followed was almost identical with that described in the experiment just reported. Each letter-group was repeated four times instead of three as in the case of the words. 'Dummy' combinations were used in the visual presentation. Practice in rating 'dummy' groups was given before the experimental material was used. In the reproduction list the following 'spelling' mistakes were permitted: (1) substitution of one letter for another, (2) reversal of two letters, (3) reversal of three letters. A study of the relation of these errors to verbal imagery will be reported in another paper.

It will be observed that no vowels are included. This reduces the imagery range very materially since imagery for them is relatively high (3). However, previous unpublished work of the writer showed conclusively that letter-groups containing vowels are remembered very much more easily than those composed only of consonants, due no doubt to the fact that they lend themselves more readily to pronunciation and are richer in associations. This combination of circumstances makes the interpretation of results unusually difficult so that it was decided to use consonants only.

The 251 subjects who took part in the experiment were divided into two approximately equal groups. Coefficients of correlation between the two sets of figures for memory, between the two for imagery, and also between those for imagery and memory were calculated. Pearson r for the scores for memory was $+ 0{\cdot}90 \pm 0{\cdot}02$ ($N = 17$). For the two sets of values for mean verbal imagery it was $+ 0{\cdot}92 \pm 0{\cdot}02$ ($N = 17$). Between mean verbal imagery and memory r was $+ 0{\cdot}47 \pm 0{\cdot}13$ ($N = 17$; 251 and 251 *Ss*). Corrected for attenuation the coefficient would lie between $+ 0{\cdot}49$ and $+ 0{\cdot}51$.

Table VI. *Comparison of letter-groups in respect of average verbal imagery claimed and number of times recalled.*

Position of group in range of imagery	Average claimed imagery		Average number of times each group was recalled	
	Arithmetic mean	Median	Arithmetic mean	Median
First 8	3·4	3·45	83	80
Last 9	2·9	3·0	73	66

The first eight letter-groups on the imagery scale were compared with the last nine in respect of memory. Table VI contains the relevant data. Of the first eight groups three equal or fall below the median memory score of the last nine, of the last nine three exceed the median memory score of the first eight.

(3) *Problem* 3.

In order to find if significant difference in the correspondence between imagery and retention exists between words or letter-groups in the first and last thirds of the reproduction lists these portions were treated separately. The coefficients and averages found are reported in Table VII.

Table VII. *Comparison of coefficients and averages obtained from the first and last thirds of the reproduction lists.*

				First third		Last third	
Experimental material	Type of imagery involved	No. of subjects	Total No. of words or letter-groups in each third	Pearson r between imagery and retention	Mean claimed imagery	Pearson r between imagery and retention	Mean claimed imagery
Words	Concrete Visual	128	558	0·45 ± 0·08 ($N = 45$)	3·5	0·33 ± 0·09 ($N = 45$)	3·3
Words	Visual Verbal	117	643	0·17 ± 0·10 ($N = 45$)	3·1	0·44 ± 0·08 ($N = 45$)	3·2
Letter-groups	Visual Verbal	247	444	0·65 ± 0·09 ($N = 17$)	3·1	0·08 ± 0·16 ($N = 17$)	2·9

It is apparent that no definite tendency emerges here. In the former experiment "Memory and mental imagery" (1) no difference could be detected between the first and last thirds in this respect. This problem requires further investigation.

(4) *Problem* 4.

When one turns from the mean image ratings assigned by sufficiently large numbers of subjects to a consideration of the imagery claimed by individual subjects, serious difficulties are encountered. Indeed, the technique just used was devised by the writer in an attempt to escape from the pitfalls of individual introspection. Two of these pitfalls are (*a*) carelessness or incompetency on the part of the subject who is rating his imagery, (*b*) constant tendency to exaggerate or under-rate the clarity of imagery. It has been shown (2) that when blurred photographs are rated as *pretended* images there is consistent tendency to exaggerate or minimize the estimates. This tendency is distributed roughly according to the normal frequency curve[1]. An attempt to correct for tendency to exaggerate or minimize was unsuccessful. However, if it be accepted that this tendency has a normal distribution one could treat the mean imagery of a sufficiently large number of *subjects* as the true imagery of a single subject. Unfortunately, it is difficult to secure sufficiently large numbers of subjects in any imagery range. The figures reported in connection with Problem 4 are open to this criticism.

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[1] Vide, *Am. J. Psychol.* 1931, XLIII, 220. The titles of Fig. 1, p. 220 and Fig. 2, p. 225 should be interchanged.

A device is suggested for elimination of careless or incompetent raters. This device has not been fully investigated experimentally and it is employed in connection with this problem to illustrate one method of use. Although its efficacy has not yet been demonstrated experimentally it can do no harm here—other than reducing the number of available subjects—and it may be of some value.

It is clear that worthless ratings, considered apart from exaggeration or minimization, are not necessarily detected by reliability coefficients since they may be perfectly consistent in their worthlessness. The writer has shown that visual terms, items in pictures, geometrical and nonsense pictures (4) and letter-groups (3) may be arranged in a fairly constant order in respect of the mean images formed of them by groups of subjects. This property may possibly be used in the appraisal of ratings.

The highest and lowest six letter-groups in the range of mean images were selected. These were respectively: cbl (3·8), bnc (3·6), plt (3·5), lvc (3·4), cjv (3·4), lht (3·3), and nlj (2·9), zwf[1] (2·8), xgt[1] (2·8), dqw (2·8), qfg (2·7), wqg (2·6). Table VIII contains data for estimation of the significance between the mean claimed imagery for the lowest member (lht) of the group with high imagery and the highest (nlj) of the group with low imagery.

Table VIII. *Arithmetic means and standard deviations of the letter-groups* lht *and* nlj.

Letter-group	No. of subjects	Arith. mean	σ dis.	σ diff.
lht	251	3·3	1·12	·093
nlj	251	2·9	0·96	

The ratio of the difference between the means to the standard deviation of this difference is $\frac{0{\cdot}4}{0{\cdot}093} = 4{\cdot}3$. Thus the difference in mean claimed imagery for the two letter-groups closest to each other in this respect is outside the operation of chance. For the high and low groups the median claimed images are 3·45 and 2·80 respectively.

Although a group of 251 subjects has rated each of the images of one set of six letter-combinations significantly higher than each of the images of a second set, it is not to be expected that this condition would hold rigidly in the case of any particular subject. For example, a certain letter-group, the average image of which is low, may be the initial letters of the name of one's friend, or even the call letters of a radio station[1]. A subject noticing this coincidence would probably have less trouble in evoking the image than another for whom the letters were without special significance. However, it seems quite plausible to suppose that a subject whose total ratings for the six 'low groups' was equal to or greater than his total for the six 'high groups' would be incompetent.

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[1] These groups although 'dummies' were rated as consistently as the others.

For obvious reasons this technique is more applicable to material such as visual verbal imagery and imagery for pictures or nonsense figures than for concrete images provoked by a word stimulus, for example by the word 'horse.' Yet even in the latter case, by using discriminating groups of stimuli not only sufficiently large but also well within the experience of the subjects, the procedure would seem to be applicable.

In connection with Problem 4 the device just described was applied with the appropriate discriminating groups to subjects rating the three types of experimental material. Care was taken to adhere to the same standard of exclusion in the three cases by selecting discriminating groups whose median imagery was approximately the same as for the two sets of letter-combinations. We are quite aware that variations of the technique could be devised which would operate with greater strictness than the one employed, but we did not feel justified in further elaboration without adequate experimental basis. From ten to twenty per cent. of the subjects rating images of the three types of material were eliminated. In Table IX are reported the average number of words remembered by groups of subjects from different segments of the concrete imagery range.

Table IX. *Comparison of three groups of subjects differing in average concrete visual imagery in respect of average number of words recalled.*

No. of subjects	Subjects' range in concrete visual imagery	Arithmetic mean of imagery of group	Arithmetic mean of words remembered by each subject	Median no. of words remembered by each subject
35	3·60–4·40	3·9	14·6	15
52	2·70–3·59	3·2	12·5	12
21	0–2·69	2·3	11·7	13

There is apparently no advantage in memory for these words by an advance in mean imagery from 2·3 to 3·2. However, a further increase of 0·7 of a step appears to yield a decided superiority in memory. The standard deviation of the difference between the two means 14·6 and 12·5 is 0·66. Dividing this figure into 2·1, the difference between the means, a quotient of 3·2 is obtained. For this reason it would appear unlikely that the difference between the two means could be attributed to the vagaries of chance. It may be recalled that there was a similar marked advantage of the highest eleven words in the imagery range over words taken from the middle and lower end of the range (Table I).

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[1] This supposition is not applicable to the letter-groups under consideration.

Table X contains the corresponding data when visual verbal imagery for words was under consideration.

Table X. *Comparison of three groups of subjects differing in average visual verbal imagery in respect of average number of words recalled.*

No. of subjects	Subjects' range in visual verbal imagery	Arithmetic mean of imagery of group	Arithmetic mean of words remembered by each subject
30	3·60–4·90	4·0	16·4
45	2·70–3·59	3·1	15·5
24	0–2·69	2·1	17·2

There is no need for further statistical treatment to show that subjects whose claimed verbal imagery is high have no advantage in the matter of recall under the conditions of this experiment.

Similarly, the average number of letter-groups recalled by subjects belonging to different segments of the range of imagery was calculated. These, with other relevant data, are reported in Table XI.

Table XI. *Comparison of two groups of subjects differing in average visual verbal imagery in respect of average number of letter-groups recalled.*

No. of subjects	Subjects' range in visual verbal imagery	Arithmetic mean of imagery of group	Arithmetic mean of letter-groups remembered by each subject
72	3·50–5·00	4·0	5·56
37	0–2·50	1·8	5·59

An average advantage of over three steps on the scale of imagery affords no advantage in the average number of letter-groups recalled.

VI. DISCUSSION OF THE RESULTS OBTAINED IN CONNECTION WITH PROBLEMS 1, 2, AND 4.

The situation is apparently this—under the conditions of these experiments there is a tendency for words whose attached concrete or verbal imagery is relatively clear to be recalled more frequently than words whose attached imagery is relatively vague. This is in agreement with the results of an experiment in which was investigated the relationship between the facility with which the component items of a picture are reproduced in visual images and the frequency of mention of these items in 'reports' (4). On the other hand, with a somewhat doubtful exception in the case of concrete imagery reported under 'Problem 4' in this paper, subjects with superior imagery appear to have no advantage in the matter of recall. This apparent paradox was previously dismissed by saying that the relationships (between mean imagery for the component parts of a picture and frequency of mention in 'reports') was not causal. While this may dispose of the matter in connection with pictures or nonsense-figures its occurrence also in connection with verbal imagery raises the question again.

Some of the possibilities may be considered.

1. The positive correlations found may be true for the material used yet they are without general validity. However, the correlation has been shown to exist for concrete images aroused by words, for verbal imagery of both words and letter-groups, and also both for items in pictures and nonsense figures.

2. Let us suppose that in a given group of 100 subjects there are 30 who tend to use imaginal cues for recall. These 30 will either fall into some fairly well-defined segment of the imagery range, presumably the highest, or else they will be distributed uniformly throughout the range. It is possible to submit the former hypothesis to the test of experiment. Two groups of subjects whose mean imagery for the 17 letter-combinations was between 0 and 2·5 and between 3·5 and 5·0 were formed. If our hypothetical group of subjects who tend to employ imaginal cues in recall should, on the average, belong to the highest portion of the scale of imagery then there should be a disproportionate number of them in the range 3·5 to 5·0 as compared with that from 0 to 2·5. The six letter-combinations whose mean claimed imagery is highest, series *A*, and the six whose mean imagery is lowest, series *B*, were selected. A comparison was made between the average number of times letter-combinations from each of the two sets were remembered. These averages are given in Table XII.

Since by our hypothesis there is a greater proportion of those who use imaginal cues among the 72 than among the 37, and since the letter-combinations of high attached imagery are easier to remember than those of low, a wider spread between the mean number of letter-groups remembered from series *A* and series *B* would be expected in the case of the 72 subjects than in that of the 37. There is apparently no support for our hypothesis. The slight difference in spread, viz. 0·16, must be attributed to chance variation. There remains the possibility that our group of subjects which tends to employ imaginal cues for recall is distributed uniformly throughout the whole range of imagery. This supposition would be consistent with our findings.

Table XII. *Comparison of the average number of letter-combinations from high and low imagery series recalled by subjects of high and subjects of low imagery.*

No. of subjects	Range of subjects' imagery	Mean of subjects' imagery	Median imagery of 'highest' six letter-combinations (series A)	Median imagery of 'lowest' six letter-combinations (series B)	Mean no. of letter-combinations remembered of series A	Mean no. of letter-combinations remembered of Series B
72	3·5–5·0	4·00	3·40	2·85	2·31	1·40
37	0–2·50	1·80	—	—	2·05	1·30

3. Another possibility which would also be consistent with the facts is that there is no one group of subjects which tends to use imaginal cues for recall. The cue for recall varies from word to word and from subject to subject. We are most attracted by this explanation.

VII. Conclusions.

1. There is a small but significant positive correlation between the mean concrete imagery attached to visual words by a group of subjects and the number of times these words are recalled by the group. This confirms a previous finding.

2. This correlation was also found to exist between visual verbal imagery for words or letter-groups and memory for them.

3. The correspondence between memory and imagery is more marked in the first third of the reproduction list than in the last third when concrete imagery and visual verbal imagery for letter-groups were under consideration. There is slight evidence of reversal of this finding for visual verbal imagery when words were employed.

4. (*a*) There is some evidence that groups of subjects claiming high concrete imagery recall more words than groups whose claimed imagery is low.

(*b*) This was not supported by the results of two experiments involving verbal imagery, nor by previous work of the writer concerning imagery for details in pictures.

The apparent contradiction between conclusions 1 and 2 on the one hand and 4 (*b*) on the other is discussed in the body of the paper.

5. It is confirmed that words have an order of 'memorial precedence.' This is true also of letter-combinations and demands consideration in experiments on memory involving nonsense syllables. The order depends considerably on the method of presentation.

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(3) —— (1932). "Factors influencing visual imagery for letter-groups." *Am. J. Psychol.* Not yet published.

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(*Manuscript received* 16 *March*, 1932.)

PUBLICATIONS RECENTLY RECEIVED.

Voice and Personality. By T. H. Pear. London: Chapman and Hall. 1931. Pp. 247 + xi. 10*s.* 6*d.* net.

For the first time in history the whole population is becoming, as Americans would say, 'voice-conscious,' owing to the extensive development of broadcast speeches and radio drama, which eliminate all the customary non-auditory cues or methods of human communication. The greater part of Prof. Pear's book consists of an entertaining and vivid account of the various aspects of speech which are thus unavoidably brought to our notice—cultural distinctions, dialects, and the visual, synaesthetic, or other processes by means of which listeners interpret the spoken word. Some experiments in this field, carried out with audiences ranging up to more than 4000 in number, illustrate the remarkable diversity of such interpretations. One wishes that the author would expand his consideration of the purely psychological problem which constitutes the title of the book. Three main factors are involved: (1) The underlying traits of the individual speaker, which Pear calls his *character*. (2) The more superficial, biosocial behaviour, including the intonation, the dynamics, and the style of speech, also other forms of gesture. Pear shows how largely these are conditioned by social training and conventions; and he includes them under the term *personality*. (3) The impressions made by the personality on the minds of the observers or listeners. We are all too ignorant of the relations between these factors. The book should certainly stimulate us to investigate them scientifically.

The Meaning of Music. By C. C. Pratt. New York: McGraw-Hill. 1931. Pp. viii + 253. $2.50.

This short book is very welcome, since one so rarely finds a writer on the psychology of music, like Prof. Pratt, who is not only a psychologist but also an enlightened musician. It will have little 'popular' appeal, for it includes many highly technical digressions into relevant psychological problems; but it is certainly of importance as a contribution to musical aesthetics. The author seems to adopt the 'characterizing attitude' in his own appreciation of music; hence his aesthetic theory consists largely in an attempt to prove that emotional qualities are objective properties of the music itself, and that 'motion' or 'highness' and 'lowness' of pitches are not figurative, associative, terms, but are intrinsic characteristics of the notes. Thus, while he dismisses the claims of those who consider music to be merely the expression of extra-musical emotions, his view-point is much broader than that of the pure formalist such as Hanslick. There are also interesting sections on previous aesthetic theories, on quarter-tone music, and a devastating demolition of the so-called tests of musical talent.

The Physical Mechanism of the Human Mind. By A. C. Douglas. Edinburgh: E. and S. Livingstone. 1932. Pp. ix + 251. 15*s.*

The aim of this book is "to demonstrate that by taking full advantage of the latest advances in physiological knowledge the gaps left by Associationism may be filled, the criticisms and requirements of Gestaltism may be met, and a complete scientific theory of Mind may be presented upon the materialistic basis which Behaviorism rightly demands." Dr Douglas therefore discusses a wide range of questions in neurology, psychology and philosophy. The most important part of the book is that dealing with the physiological mechanism of such processes as perception and association, but the treatment of these topics is too brief to be adequate, and it is not always clear where observed fact ends and speculation begins.

Dr Douglas writes agreeably and the book is well produced.

The Science of Man in the Making. By E. A. KIRKPATRICK. London: Kegan Paul, Trench, Trübner and Co., Ltd. 1932. Pp. xv + 396. International Library of Psychology. 15*s.* net.

The scope of this book can best be indicated briefly by giving its chapter headings. These are: Nature and Methods of Science; Man as an Inhabitant of the Earth; Varieties of Human Species and their Modes of Living, or Anthropology and Ethnology; How Life is Preserved, or Physiology and Hygiene; Improving the Human Species, or Eugenics and Euthenics; Avoiding Waste, or Economics; Means of Control, or Political Science; How Man Behaves, or General Psychology; Personality Differences, or Individual Psychology; Behaviour in Relation to Others, or Social Psychology; Organised Group Living, or Sociology; Changing Human Beings, or Education; Man and the Unseen World, or Religion; Regulation of Human Interaction, or Morals; Man, the Master of Life, Developing a Science of Ethical Living. It will be seen that an immense range is covered. Hence the discussions are bound to be of a very general nature, and every specialist will tend to be left somewhat unsatisfied concerning the treatment of his own particular field. Nevertheless the writing is almost uniformly pleasant, tolerably concise and not over-dogmatic, and the book is a very good one for general reading. Much of it almost inevitably seems to be a statement of the obvious, but the whole should serve the purpose for which it was written and help a reader towards a general orientation in relation to the various related sciences which lead up to or deal with the development and control of human behaviour. Every chapter has an appendix suggesting further more specialized reading. The choice of books of a more specialist nature is judicious, though most of the authors recommended are American. On the whole Mr Kirkpatrick has succeeded excellently in his aim and has produced a readable and useful survey.

The Affective Consciousness. By HALBERT HAINS BRITAN. New York: The Macmillan Company. 1931. Pp. ix + 391. 15*s.* net.

The author of this book gives a descriptive treatment in popular language, without much resort to theory, of the development and differentiation of the emotions from the primary affective states of pleasure and pain. He stresses the vital importance and the impellent effect of the emotions in the adaptation of man to his environment. The simpler emotions are dealt with straightforwardly and clearly, if a little obviously. But we cannot feel that much light is thrown upon those difficult topics, the so-called ethical, religious and aesthetic emotions.

The Laws of Human Nature. By RAYMOND H. WHEELER. Contemporary Library of Psychology. London: Nisbet and Co., Ltd. (Cambridge University Press.) 1931. Pp. xiv + 232. 5*s.* net.

The author of this book has drawn up a system of eight laws of nature which are said to regulate both physical and biological events. He stresses the universal bearing of configurational principles and the importance of organized wholes in human behaviour. Too much space, however, is devoted to physical systematization, and the treatment of psychological phenomena, though interesting and suggestive as far as it goes, is thus necessarily somewhat superficial.

The Mind in Action. By A. CAMPBELL GARNETT. Contemporary Library of Psychology. London: Nisbet and Co., Ltd. (Cambridge University Press.) 1931. Pp. xii + 226. 5*s.* net.

This thoroughly sensible and well-written book deals clearly and straightforwardly, if without any great originality of thought, with the conative aspect of consciousness, and with the various theories of instinct and sentiment. The treatment of the problem of values is necessarily brief, but not so superficial that the student, as well as the non-technical reader, may not derive interest and instruction from it.

Psychology and Profits. By DONALD A. LAIRD. London: Williams and Norgate, Ltd. 1932. Pp. 175. 6*s*. net.

A few interesting observations are embedded in this mass of clichés and disconnected chit-chat; but it is difficult to see the object of publishing such a book.

The Unconscious in Life and Art. By S. HERBERT. London: George Allen and Unwin, Ltd. 1932. Pp. 252. 6*s*. net.

This book is made up of eight essays on the following topics: Reason and Unreason; Psycho-analysis and Sex; Psycho-analysis and Married Life; The Self and Society; The Genesis of Conscience; Symbolism; Phantasy and Thought; The Romantic Spirit. As will be guessed from these titles the author writes as a convinced psycho-analyst. While they have nothing very new to say the essays are uniformly interesting and are very clearly and pleasantly written. They are, moreover, entirely free from difficult technical detail, and can be read, no doubt to his enjoyment and profit, by any discriminating reader, whether he professes to be a psychologist or not. Whatever may be thought of the positive doctrines advanced, some of the criticisms of other writers are interesting and shrewd.

Invention and the Unconscious. By J. M. MONTMASSON. Trans. with a Preface by H. S. HATFIELD. London: Kegan Paul, Trench, Trübner and Co., Ltd. 1931. Pp. xxiv + 333. 15*s*. net.

This book is one of the International Library of Psychology. It is concerned with the unconscious in the older fashioned sense, as "the residue of a consciousness which has reached its minimum of attention." There are two parts: the first concerns the Domain of the Unconscious in the Sciences, and the second the Genesis of Invention under the Influence of the Unconscious. The author believes that at every stage of constructive thinking the unconscious, in his sense, prepares the way for conscious realization and elaboration. No intellectual reaction is merely cognitive; it is affective and conative as well and in all three of its characteristics the unconscious has its essential part to play. Many and diverse illustrations are given, but more stress is laid upon the fact of unconscious interplay than upon the manner in which it is active. The book has much to say that is of interest though in style it is rather scrappy, and at the same time rather wordy. Certainly the material that it presents by way of illustration provides numerous difficult but fascinating problems.

The Case of Mrs A. By ALFRED ADLER. Individual Psychology Publications, No. 1. London. 1931. Pp. 48. 2*s*. net.

Anorexia Nervosa. By W. LANGDON BROWN, F. G. CROOKSHANK, J. C. YOUNG, GEORGE GORDON and S. C. BEVAN BROWN. Individual Psychology Publications, No. 2. London. 1931. Pp. 63. 2*s*. 6*d*. net.

Individual Psychology and Sexual Difficulties. By ALFRED ADLER, RUDOLF DREIKURS, ERWIN WEXBERG, FRAU DR HERVAT, J. C. YOUNG and F. G. CROOKSHANK. Individual Psychology Publications, No. 3. London. 1932. Pp. 70. 2*s*. 6*d*. net.

Individual Psychology and Psychosomatic Disorders. By W. LANGDON BROWN, O. H. WOODCOCK, J. C. YOUNG, S. VERE PEARSON, M. B. RAY, M. ROBB and F. G. CROOKSHANK. Individual Psychology Publications, No. 4. London. 1932. Pp. 69. 2*s*. 6*d*. net.

Individual Psychology and Social Problems. By ALFRED ADLER, W. BÉRAN WOLFE, C. L. C. BURNS and J. C. YOUNG. Individual Psychology Publications, No. 5. London. 1932. Pp. 60. 2*s.* 6*d.* net.

These monographs, all published by the C. W. Daniel Company, are quarterly publications of the Medical Society of Individual Psychology, and are devoted to a discussion and exposition of problems of conduct along the lines initiated by Alfred Adler. They deal with a great variety of topics, though, as a rule, not-very fully with any one problem. They are generally clear and attractively written, and though they are of the nature of propaganda for a particular point of view, they contain much that will both interest and help the ordinary reader.

On the Nightmare. By ERNEST JONES. London: The Hogarth Press and the Institute of Psycho-analysis. (International Psycho-analytic Library, No. 20.) 1931. 21*s.* net.

Discounting the old popular view that nightmares were sexual assaults on the part of lewd demons which were strongly resisted by the victims, and the 'current medical view' which ascribes them to physiological disturbances, Dr Jones shows that the origin of the nightmare—the three cardinal features of which are agonizing dread, sense of oppression or weight at the chest which alarmingly interferes with respiration, and conviction of helpless paralysis—lies in the repressed incestuous desires of the dreamer. He goes on to show what an enormous influence dreams, and especially terror dreams, have had on the elaboration of various beliefs, of the ideas of fabulous or disreputable supernatural beings, of witches and vampires, of werewolves and of the devil, though the actual formulations of the superstitions were influenced by many factors, chief among which were certain features in the attitude of the church. The almost universal beliefs in a soul that survives death, and in a deity, apparently have the same origin. "It is hard," Dr Jones says, "to avoid the conclusion that religion is, in its essence, one of the means...of helping mankind to cope with the burden of guilt and fear everyone inherits in his unconscious from...the primordial conflict over incest."

In addition there is an excursion into etymology—Mare and Mara.

A most erudite, yet eminently readable book, it is a distinct contribution to psychoanalytical literature and one which those who are not psycho-analysts can enjoy.

The Technique of Psycho-Analysis. By HANS VON HATTINGBERG. Trans. by A. EILOART. London: The C. W. Daniel Co. 1932. Pp. 136. 5*s.* net.

On the whole this should prove a useful book. It gives a readable and intelligible account of psycho-analytic procedure, and at the same time is not very narrowly tied down to the views of any particular school. There is no doubt a danger that all such accounts may lead to unskilled treatment by amateurs, but the undesirability of this is clearly stated, and fairly definite indications are given of the sort of training that is necessary and possible.

Personality and Social Adjustment. By ERNEST R. GROVES. 2nd ed. London: Longmans, Green and Co., Ltd. 1931. Pp. xi + 353. 7*s.* 6*d.* net.

This book gives a popular and somewhat unscientific account of the bearing upon sociology and education (in the broader sense) of recent findings and theories in the psychology of the emotions and conative tendencies. Particular attention is paid to methods of building up character. This second edition differs from the first chiefly in its treatment of the instincts; this category of behaviour is now regarded as much more limited in its extent than was formerly supposed.

Homicide in the United States. By H. C. BREARLEY. University of North Carolina Press. (London: Humphrey Milford.) 1932. Pp. x + 249. 17*s.* 6*d.* net.

Dr Brearley has produced an extremely clear and well-analysed study. He has collected extensive statistics and he presents the facts with a minimum of well-directed comment. The general homicide rate in the United States is very high compared with that of other countries, but its very uneven distribution may readily lead to false impressions in regard to certain areas. Many causes have been assigned for the relatively high rate and these are briefly reviewed and discussed. Very interesting sections deal with "gang slayings," "negro homicide," and "seasonal variations in homicide." Many tables are given and there is a most useful bibliography. The book will be of great service to the social worker everywhere.

A Study in Aesthetics. By MILTON H. BIRD. Harvard University Press. (London: Humphrey Milford.) 1932. Pp. x + 117. 5*s.* 6*d.* net.

An introduction dealing with ancient and modern aesthetic theories and experiments leads up to an account of some interesting drawing experiments. The author's general problem was to investigate the relation of intelligence to ability in drawing. He presents evidence of a positive relationship and draws some conclusions with regard to the methods that should be adopted in teaching children to draw. The treatment throughout is competent and concise, especially in the introductory sections.

Atti del VII Convegno di Psicologia sperimentale e psicotecnica, tenuto in Torino, Novembre 1929. Bologna: Nicola Zanichelli. 1932. Pp. 234.

These proceedings, published by the Italian Psychological Society, contain abstracts of forty-eight papers. Nineteen of these deal with problems in general psychology, most of them, but not all, of an experimental nature. Nineteen of them concern applied psychology, mainly in educational and industrial fields. Five discuss problems of abnormal psychology, and the rest are on diverse topics and are unclassified. The whole is a witness to much well-directed psychological activity in Italy.

The Mind: A Key to the Interpretation of Psychical Phenomena. By G. E. REHBERGER. Published by the Author: Phoenix, Maryland. 1931. Pp. 214.

The author claims that this book has been inspired by his dream experiences. It does not, however, contain anything very novel. A dream is a reaction to a stimulus directed by suggestion. This general notion is expounded and applied to numerous phenomena of abnormal psychology.

The Oxford Group Intelligence Test. By G. PIRRIE WILLIAMS. London: G. Harrap and Co., Ltd. 1932. Pp. 4. 6*d.* (5*s.* for 25).

The tests included are: (1) perception of difference with words; (2) perception of difference with digits; (3) analogies; (4) pattern; (5) classification of letters and forms. No norms are given.

La Philosophie de Fontenelle ou le sourire de la Raison. Par J. R. CARRÉ. Paris: Félix Alcan. 1932. Pp. 705. 70 fr.

This is a most excellent, clearly expressed and well-arranged study. Any psychologist will read with pleasure and profit especially the second part of M. Carré's exposition. For here Fontenelle's reflections on human nature are set forth. The warm belief in the significance of human reason which in no way leads Fontenelle to overlook the basic importance of the emotional side of life is an excellent corrective for many of the one-sided views prevalent to-day. The topics dealt with here are: the whole man; the passions; reason and instinct; the history of reason; the functions of reason; the gateway of reason, and finally M. Carré discusses the probable sources of Fontenelle's views. The rest of the book is more philosophical, less psychological. The writing throughout is most clear and scholarly.

Fontenelle: De l'Origine des Fables. Édition Critique, avec une introduction, des notes et un commentaire. Par J. R. CARRÉ. Paris: Félix Alcan. 1932. Pp. 104. 20 fr.

Fontenelle's essay, first published in 1686, raises a number of psychological points which are still of great interest, and this carefully edited and annotated edition will be found of considerable value by the psychologist with a taste for historical research.

Old Beliefs and New Knowledge. By WILLIAM RUSSELL. London: John Bale, Sons, and Danielsson, Ltd. 1932. Pp. vi + 208. 5*s*. net.

This is in the main a very condensed comparative study of the development of religious beliefs. There is not a great amount of psychology in it, and what there is is mainly built upon the view that "Palaeolithic man with the big brain showed the possession of three brain areas, namely, (1) the mechanical, (2) the organizing, (3) the artistic and the psychic. All the developments of religious belief are due to the functioning of the third 'area.'"

The Activity Movement. By CLYDE HISSONG. Educational Psychology Monographs, No. 30. Baltimore: Warwick and York. 1932. Pp. ix + 122. $2.10.

By "the activity movement" is meant the tendency which has overspread much current educational practice of attempting to allow the greatest possible freedom to the individual child. Types of activity schools are discussed, the basis of the movement in Rousseau's views is considered, and an attempt is made to determine the significance that must be placed upon 'freedom,' both as an idea and as a practice, in modern education. In general the book tries to show the philosophic foundation and also the probable development and value of "the activity movement."

Twenty-Five Years. A Memorial Volume in Commemoration of the Twenty-fifth Anniversary of the Vineland Laboratory: 1906–31. Edited by EDGAR A. DOLL. New Jersey: The Training School at Vineland. 1932. Pp. 135.

Various workers here give an account of the work attempted and achieved in the study of the feeble-minded at the Vineland Institution. The whole is dedicated to Dr Henry H. Goddard. A bibliography of the publications of the Laboratory is given.

El Amor y El Genio. Por CARLOS SFONDRINI. Madrid: Poblet. 1932. Pp. 332. 10 pesetas.

The author of this considerable monograph is a professor of philosophy at Buenos Aires. The book deals comprehensively with love as a factor in the intellectual life of man. The first part is largely physiological and deals with human sexual reactions mainly in their relation to outstanding intellectual activity. The second part consists of a prolonged, but interesting study of the psychology of love. The third part considers the main conclusions already arrived at in relation to the life and productivity of the genius. The whole is a serious and sustained attempt to discover the parts played by love in the life of man, both in its normal and in its perverse expression, and to show why, in one form or another, love has often been of predominant importance in the life of the man of genius. The author has been influenced mainly by Italian and French writers.

Modern India Thinks. A Symposium of Suggestions on Problems of Modern India. Compiled by KESHAVJEE R. LUCKMIDAS. Foreword by KANAIVALAL H. VAKIL. Bombay: D. B. Taraporevala Sons and Co. 1932. Pp. xix + 298. Rs. 6.

This book consists of a large number of quotations, many of them very brief, setting forth some of the views of contemporary Indian thinkers on: Civilisation, Culture, Religion, Aesthetics, Ideals, Education, Work, Wisdom, Character, Nation-Building and Social Service. The collection may be of some service to the social psychologist who is interested in contemporary groups, but he will find interpretation difficult.

Berkeley's American Sojourn. By BENJAMIN RAND. Cambridge, Massachusetts: Harvard University Press. 1932. Pp. xi + 79. $2.00.

Consciousness and its Purpose. By AURA MAY HOLLEN. Hollywood, California. The Keats Publications. 1932. Pp. 246.

Hauptsätze der Metaphysik. Von BRANISLAV PETRONIEVICS. Heidelberg: Carl Winter. 1930. Pp. 82.

L'Écriture et le Subconscient: Psychanalyse et Graphologie. Par PIERRE MANARD. Paris: Félix Alcan. 1931. Pp. 172. 20 fr.

Philosophy and the Ordinary Man. The Presidential Address to the British Institute of Philosophy. By Sir HERBERT SAMUEL. London: Kegan Paul, Trench, Trübner and Co., Ltd. 1932. Pp. 38. 1*s*. 6*d*. net.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

GENERAL MEETINGS.

June 25, 1932. Held at the National Institute of Industrial Psychology, by invitation of the Institute.

Papers:

"An Experimental Enquiry into the effects of 'Practice' and of 'Training,' in Manual Operations." By Mr J. W. Cox, D.Sc., Research Fellow of the Institute.

"A Critical Examination of Recent Work on Perseveration." By Mr B. Babington Smith, B.A., Voluntary Research Worker of the Institute.

Demonstrations:

Selection Tests. By Mr A. H. Seymour, B.Sc.

Vocational Guidance—Test Material and "Psychographic Profiles." By Mr A. Macrae, M.A., M.B., Ch.B.

Tests for Motor Drivers. By Mr D. F. Vincent.

A Borstal Investigation. By Mr T. A. Rodger, B.A.

SECTIONAL MEETINGS.

Medical.

June 22, 1932. "Hystero-Epilepsy." By Dr C. Worster-Drought.

Aesthetics.

July 1, 1932. Discussion Meeting: "The Nature of Aesthetic Experience." Opened by Miss E. M. Bartlett, B.A.

DR VICTORIA HAZLITT
1887–1932

VOLUME XXIII JANUARY, 1933 PART 3

VICTORIA HAZLITT, 1887–1932.

BY A. W. WOLTERS.

DR HAZLITT came to the study of psychology by way of philosophy. Her student years were spent at Bedford College, London, and after graduating in 1910, she obtained in 1912 an appointment in the State University of Colorado. Returning from America in 1914, she joined the staff of Bedford College, where she continued to work until her death. She was a gifted teacher who exacted a high measure of endeavour alike from her pupils and herself. Her teaching experience was enriched by the conduct of extra-mural classes, and, always ready to obtain psychological material from every side, she profited by the wider contacts which this work entailed. Psychology was for her no cloistered academic pursuit. An enthusiastic worker in the laboratory, she knew equally how to find data in the observation of a group of labourers at work. Psychology meant to her the study of real human beings, rather than of the artefacts around which psychological literature has too often centred. Further, having devoted herself to the prosecution of the science, she interpreted generously the responsibilities of her calling, and was ready to serve at all times and in any manner. It is proper to recall here the services which she rendered to the British Psychological Society. She was for many years a member of its Council, and acted for some time as secretary of the Committee for Research in Education. To the affairs of the Society she gave her time and ability fully (she could not give otherwise) and her practical common-sense served it well.

Her first published work, "The Acquisition of Motor Habits" (1), was a study of animal behaviour which revealed her gifts for patient investigation and for the interpretation of results. She gave here the first indication of her general psychological outlook in a vigorous rejection of any quasi-mechanistic theory of learning. She regarded the processes of animals' learning as being thoroughly continuous with those of human beings, and as revealing intelligence and insight comparable with those of man. Consistently with this view she was prepared to ascribe the transfer of skill shown by rats to a factor which, however lowly and indeterminate it might be, must be termed ideational. The readiness of the trained rats to deal with the problem presented by a new maze showed, she held, an appreciation of the general situation which must depend upon a consciousness of meaning, and this, since it goes beyond immediate sensation, can and should be called ideation. To interpret their behaviour thus did not mean that it was necessary to ascribe explicit conscious process to the animals, for even in man ideation is not to be identified with conscious processes. At a later date she turned to account her experience of the pitfalls encountered in the study of animal behaviour in an article criticizing later experimental work (2).

Dr Hazlitt carried her analysis of mental process considerably further in her book on *Ability* (3) (for which she was awarded the degree of D.Litt. in the University of London). Here she advanced views which, though related to current discussions, presented certain original characters. Taking intelligence to be the 'problem solving organization,' she distinguished two factors in intelligent activity, the 'drive,' and the 'evocation of relevant elements.' Thus she emphasized the conative aspect of intelligent behaviour, and suggested that the so-called special abilities were, in fact, only hypostatizations. The observed fact is the degree of success in a given field. It was Dr Hazlitt's view that the differences shown by an individual in several fields of activity were consequent upon differences in the material involved. The apparent existence of a special capacity is to be accounted for by the possession of adequate previous experience in that sphere, together with strong conative tendencies in relation to it. The cognitive material is acquired by reason of such tendencies, and in return shapes and directs them. If there are special factors leading to the successful handling of problems in one sphere rather than in another, they are to be sought on the side of instinct and affect. On the cognitive side there is one factor only, and this is perfectly general, though varying in degree from one person to another. This factor, through which the available mental material is rendered serviceable, she identified with the 'degree to which experiences are confluent.' With homely but telling examples the author sought to show that the organization of tendencies upon which efficient behaviour depends is brought into being by the reciprocal interaction of more elementary experiences. Similarly, the relevant organization becomes operative through a synthesis of itself with the task. "There seems a perfect determination of what is brought to bear by the exact form and detail of the immediate situation. This determination is, of course, always in relation to the past experiences and their integration[1]." The whole resultant from confluence or integration is not explicable entirely by enumeration of the components and their relations, since in entering into relation the elements modify each other. In her resolute rejection of every type of psychological atomism she was in sympathy with the Gestalt school, but did not commit herself to their views. She studied current theories closely, and learned from them without submitting to them. In general she suspected elaborate theoretical constructions which went far beyond observed fact. Her views are in the line of descent from Ward and Stout, from whom she probably learned more than from later German writers.

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[1] From a letter of March, 1932.

The extent to which integration of experience is possible is determined from birth, and is not susceptible of increase by training or by age. The advance in capacity which is shown by mental tests applied at successive ages is due to the maturation of experience. Consequently, the results of the tests can only be taken as measures of general ability when the experiences of the subjects tested are comparable. But the growth of experience depends upon conative and affective factors. Therefore, within the field of the test material we are measuring the efficiency of the total individual, but efficiency is dependent upon environmental conditions as well as upon innate factors. In this way Miss Hazlitt endeavoured to give a coherent account of mental testing which should cover both its strength and its weakness.

Her book arose from an attempt to construct a series of tests to be applied to undergraduates when entering the university, with a view to discovering the most profitable course of study for each. Although the results were interesting and promising they failed to satisfy her exacting requirements. With characteristic courage and decision she abandoned the work on which she had been so long engaged, and turned to a field which she considered to offer more hope of solving the problem of intelligence, the field of child psychology. This did not indicate any real change of interest, for she regarded psychology as a unitary whole, and turned to child psychology as the most fruitful department for genetic study. The wide extent of her knowledge, her capacity for criticism, and her loyalty to empirical methods, were shown in further publications (4, 5), but the accident of last April prevented her from reaping the reward of long and patient endeavour. Dr Hazlitt had already rendered substantial service to English psychology, but the full measure of her powers had still to be displayed, and her most important work remains undone. It is hoped that it may be possible to publish later the completed sections of the book upon child psychology upon which she was engaged.

She bore her learning lightly, and was slow to obtrude her views on others: she was incapable of self-assertion. But she jealously preserved her independence of judgment. She had retained her interest in philosophy, which coloured her psychological views and led her to insist upon the organic unity of the individual as the fundamental fact of mental life. Philosophical and psychological views reflected the personality of one who found interest in many aspects of life. Her deep sense of values was balanced by a lively humour, and the tolerance and sympathy which flow from these traits marked all her relationships. These were the qualities which made her friendship so richly valuable.

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THE PERIPHERAL PERCEPTION OF MOVEMENT.

By M. D. VERNON.

(*From the Cambridge Psychological Laboratory.*)

I. *Introduction* (pp. 209–10).
II. *Description of experimental method* (pp. 211–15):
  (1) *Apparatus* (pp. 211–13).
  (2) *Experimental series* (pp. 213–14).
  (3) *Subjects* (p. 215).
III. *The results of the experiments* (pp. 215–28):
  (1) *Central form and peripheral movement perception* (pp. 215–21).
  (2) *Central and peripheral movement perception* (pp. 221–3).
  (3) *Peripheral form and central movement perception* (pp. 223–5).
  (4) *Peripheral form and movement perception* (pp. 225–8).
IV. *Discussion and conclusions* (pp. 228–31).
V. *Summary* (p. 231).
References (p. 232).

I. Introduction.

It has been stated by Exner (5) that the periphery of the retina acts as a specially differentiated organ for the perception of movement. It is indeed a matter of common observation that a moving object seems to stand out with special clearness when it appears in the peripheral visual field; moreover, it arouses an immediate tendency to turn the eyes in that direction, a tendency often emotionally toned with surprise and even fear. Yet it has been shown by Basler (2) and Ruppert (14) that the threshold for movement, that is to say, the least discriminable distance moved, is actually greater in peripheral than in central vision. On the other hand, Exner (6) found that in peripheral vision movement could be perceived over a distance less than the least distance separating two stationary points which could be discriminated from each other.

The decrease in visual acuity in passing from the central to the peripheral retina is caused in some part by the summation which has been shown by Granit (7) to occur in the latter, probably as a result of the synaptic convergence of the neurones from the receptors (and also, of course, in part by the fewness of the receptors, and by the distortion produced by the lens). This summation, however, tends to favour the perception of a moving object, as has been shown by Sherrington with the scratch reflex (15), and by Adrian (1) in the eye of the conger-eel. For if a subliminal stimulus is applied at one point, and a second subliminal stimulus shortly afterwards at another point close to it, the two will summate and produce a supra-liminal excitation. Consequently a movement between two points is perceived when neither point could be perceived alone when stationary; and a movement between two points can be discriminated, although the two points when stationary cannot be discriminated. Moreover, it has been shown by Adrian (1) that when the eye is stimulated, the excitation rises rapidly to a maximum and then falls to a lower, steady level. But if the point of stimulation changes sufficiently rapidly, the excitation will always be at its maximum. Consequently the excitation produced by a moving supra-liminal stimulus will always tend to be greater than that produced by a stationary stimulus of the same intensity.

Thus it seems easy to understand why peripheral movements are relatively clear, while peripheral form percepts are blurred, vague and indistinct. But under normal conditions the peripheral movement percept must compete in consciousness not so much with the peripheral form field, but with the central form field which is clearly seen, and is in general at the focus of consciousness. How, then, can we explain this sudden irruption in consciousness of the peripheral movement percept, overthrowing the habitual dominance of the central form percept, and assuming, momentarily at least, the focal position?

To examine this conflict between peripheral movement perception and central form perception, a series of experiments was carried out in which a moving object and a stationary form were exposed simultaneously, the former in the periphery, the latter at the centre, of the visual field. The experiments were so designed that neither the forms nor the movements could in general be perceived accurately and easily; and it was hoped that some light would be thrown upon the conflict in consciousness by the effects upon accuracy of perception. Further series of experiments were carried out upon simultaneous central movement and peripheral form perception, simultaneous central and peripheral movement perception, and simultaneous peripheral form and movement perception; and the effects upon accuracy in the latter series were contrasted with the effect in the former.

II. DESCRIPTION OF EXPERIMENTAL METHOD.

(1) *Apparatus.*

The apparatus used in these experiments consisted essentially of a vertical semicircular screen of white cardboard, 6 ft. in diameter and 20 in. high. This is shown in horizontal section at *SS* in Fig. 1. The subject sat with his chin upon a chin rest *R*, facing the centre of this screen, at which was a fixation point *P* illuminated from behind. A bright form and a moving line could be presented at any point on this screen, at a height upon it level with the subject's eyes. To produce these forms, light from an 80-watt pearl lamp *K* passed through a photographic shutter *T* to a framework, shown in front elevation in Fig. 2. This carried two portions of opaque lantern slide F_1 and F_2; upon the former was scratched a vertical line, and upon the latter a form or design. The light transmitted through these slides was focused by lenses L_1 and L_2 and reflected by the rotating mirrors M_1 and M_2 to produce bright images of the form and line in the required positions upon the screen.

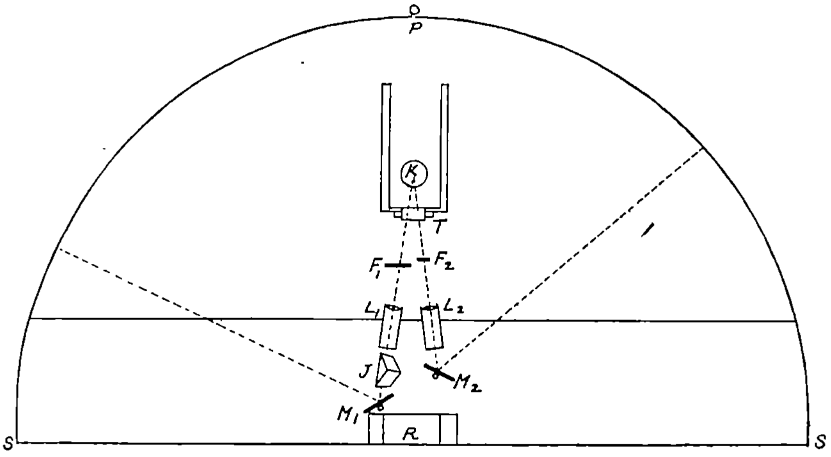

Fig. 1.

The movement of the line was produced as follows. The photographic slide F_1 was fastened to a rigid frame *AA* (Fig. 2) which could slide horizontally along rods O_1 and O_2. Also sliding along O_2 was a bar B_1 attached to the rotating wheel *W*, from the circumference of which were suspended two weights E_1 and E_2; E_1 was slightly heavier than E_2. A second bar B_2 attached to the wheel rested upon a support *C*; and a third bar B_3 joined to this support projected horizontally to rest upon the catch which opened the photographic shutter. Thus when the support C was released, the photographic shutter opened, and the wheel was released and was rotated by the weights in an anti-clockwise direction; the bar B_1 moved along the rod O_2, and moved the framework AA and the slide F_1 from right to left. The length of the movement was regulated by a stop D, the position of which was measured on the graduated glass screen G.

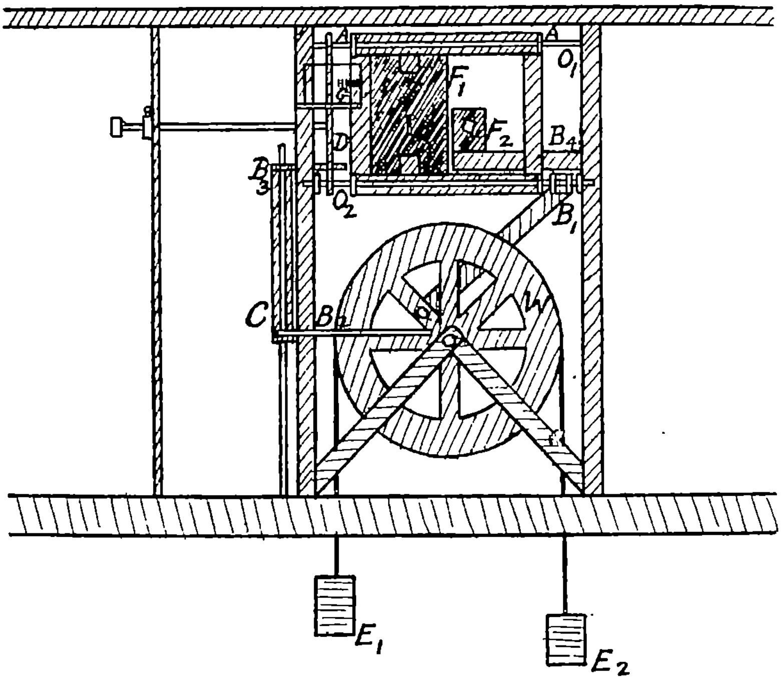

Fig. 2.

This horizontal movement of the line scratched on the slide F_1 was altered into a vertical movement on the screen by placing a right-angled prism J (see Fig. 1) in the path of the beam of light.

The stationary form F_2 was held in position on the bar B_4. In the experiments in which two moving lines appeared, F_2 was removed, and another piece of lantern slide with a vertical line scratched upon it was fixed side by side with F_2. A second right-angled prism was inserted in the corresponding position to J. Both eyes were used throughout the experiments, but the peripheral object could only be seen by one eye.

The illumination of the screen was kept constant at 0·35 foot candles

Table I. *Illuminations and dimensions of test objects.*

			Dimensions of							
			Forms				Moving line			
	Illumination in f.o.s. of		Extent		Thickness of lines		Length		Thickness of lines	
Series	Forms	Moving line	Linear	Angular	in cm.	Angular	in cm.	Angular	in cm.	Angular
1 (*a*)	0·6	1·0	1·50	56′	0·10	4′	2·75	1° 43′	0·25	9′
1 (*b*)*	1·1 0·8	1·0								
1 (*c*)* 1 (*d*)*	1·5 1·1 0·8	1·0	Large square 3·4 Small circle 0·6	2° 8′ 23′	0·10	4′	2·75	1° 43′	0·25	9′
2	—	Central 1·1	—	—	—	—	1·50	56′	0·10	4′
		Peripheral 1·0	—	—	—	—	2·75	1° 43′	0·25	9′
3	1·0	1·1	3·4, 2·45, 1·65	2° 8′, 1° 32′, 1° 2′	0·25	9′	1·50	56′	0·10	4′
4	1·0	1·0					2·75	1° 43′	0·25	9′
5	1·65	1·0	Rectangle 5·0 Small square 0·7	3° 8′ 25′	0·10	4′	2·75	1° 43′	0·25	9′

* It was necessary to vary the illumination of these forms to compensate for the varying visual acuity of the subjects.

throughout; the lines and forms were brighter (see Table I). The wall of the room above the screen was illuminated by a hidden source of light, and gave a brightness at the eye of 4·9 millilamberts (assuming the screen of the luxometer to reflect 80 per cent. of the incident light).

(2) *Experimental series.*

Five series of experiments were carried out as follows:

1. (*a*) *Forms*[1]. Forty small, conventional, fairly complex, outline forms, exposed for 1/5 sec., at the centre of the field[2]. Presented simultaneously with movements.

   Movements. Movements of horizontal line of from 0 to 4 cm. (2·5°) at 60° and 40° left and right of fixation point. One series of movements each presented simultaneously with a form; one series without.

   Rate of movement about 15° per sec. throughout.

   (*b*) *Forms.* Forty similar to those in 1 (*a*).

   Movements. 0 and 4 cm. movements only. Positions as in 1 (*a*).

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[1] Dimensions and brightnesses of forms and moving line given in Table I.

[2] During the earlier experiments of Series 1 (*a*) and 3, the subjects were required to describe the forms; but later it was found more convenient for them to draw them.

(*c*) *Form.* A square subdivided by horizontal and vertical lines into nine smaller squares; eight of these contained still smaller squares, and one a square or a circle. The circle could be exposed in each of the nine possible positions except the central one. Exposed for 1/5 sec. at the centre of the field. One series with, one without, movements.
Movements. As in 1 (*a*).

(*d*) *Form.* As in 1 (*c*).
Movements. As in 1 (*b*).

2. *Forms.* None.
Movements. Movements of one horizontal line of from 0 to 4 cm. at the centre of the field, and of a second horizontal line, through equal distances, at 60° and 40° left and right of fixation point. Exposed both simultaneously and separately.

3. *Forms.* Ten conventional, simple, outline forms (*e.g.* square, triangle, etc.), each presented in three different sizes. Five of these forms presented for 1/5 sec. at 60°, 50°, 40°, 30°, 20° to the right of the fixation point, and five presented in the corresponding positions on the left.
Movements. Movements of from 0 to 4 cm. at centre of the field. One series with, one without, forms.

4. (*a*) *Forms.* The same as in 3[1], slightly differently arranged, presented for 1/5 sec. at 40° and 30° left and right of fixation point.
Movements. Movements of from 0 to 4 cm. at 60° right and left of fixation point, on the same side of the field as the forms. One series with, one without, forms.

(*b*) *Forms.* Similar to those in 4 (*a*), but presented on opposite side of the field to the moving line.
Movements. As in 4 (*a*).

5. (*a*) *Form.* A rectangle divided into four equal rectangles, each of which was further divided into an irregular reticulate pattern, containing within it one regular square. Form exposed at the centre of the field until the subject perceived the square and reported in which of the four divisions it was situated. (Square sometimes not presented.) Time of exposure recorded by an electric marker, tracing on a drum. Form exposed sometimes with moving line, sometimes without.
Movements. Movements of horizontal line of 0 and 4 cm. only, at 40° left and right of fixation point. Not more than one movement presented during each exposure of form.

(*b*) *Form.* As in 5 (*a*), but square replaced by right-angled triangle.
Movements. Line kept stationary, or moved through 4 cm. continuously, by hand, once per second during exposure of form.

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[1] A considerable period of time elapsed between the experiments in Series 3 and 4, and the subjects did not recognize that they had already seen in Series 3 the forms shown in Series 4.

(3) *Subjects.*

The subjects of these experiments were seven educated adults all working at the Psychological Laboratory. Two of these, Subjects 6 and 7, took part in Series 4 and 5 only, and one, Subject 4, in Series 1 and 3 only.

III. THE RESULTS OF THE EXPERIMENTS.

(1) *Central form and peripheral movement perception.*

The effects of central form perception and peripheral movement perception upon each other will first be considered. Table II and Fig. 3 show the percentage number of forms and movements perceived correctly by each of the five subjects in Series 1 (*a*) and 1 (*c*) when both form and movement were seen, when movement was seen alone, and when form was seen with the peripheral line stationary.

Table II. *Effects of central perception of form and peripheral perception of movement upon each other.*

	No. of accurate judgments as percentage of all judgments											
	Central form						Peripheral movement					
	Series 1 (*a*)			Series 1 (*c*)			Series 1 (*a*)			Series 1 (*c*)		
Subject	No movement seen	Movement seen	Diff.	No movement seen	Movement seen	Diff.	Without form	With form	Diff.	Without form	With form	Diff.
1	94	91	3	100	78	12	84	67	17	89	66	23
2	35	54	–19	39	43	– 4	71	64	7	81	59	22
3	76	70	6	(33	65	–32)	81	77	4	89	98	– 9
4	94	92	2	61	57	4	74	58	16	79	73	6
5	74	74	0	(35	60	–25)	60	45	15	68	64	4

It is quite clear from Table II and Fig. 3 that the perception of central form was in no way impaired, in these series, by simultaneous perception of peripheral movement, except possibly in the case of Subject 1. Indeed, in the case of Subject 2, the central form perception in Series 1 (*a*) appeared to be improved. The reason for this seems, from his introspections, to have been that, when the line only made small movements, such as were rarely perceived, he was in a state of considerable doubt and uncertainty, which prevented him from attending to the form, or caused him to forget its shape before it could be recorded. Clear and unequivocal perception of movement in the periphery removed this state of uncertainty, and allowed him to concentrate on the form. (The form perception of Subjects 3 and 5 also appeared to be improved, in Series 1 (*c*), by simultaneous movement perception; but the number of

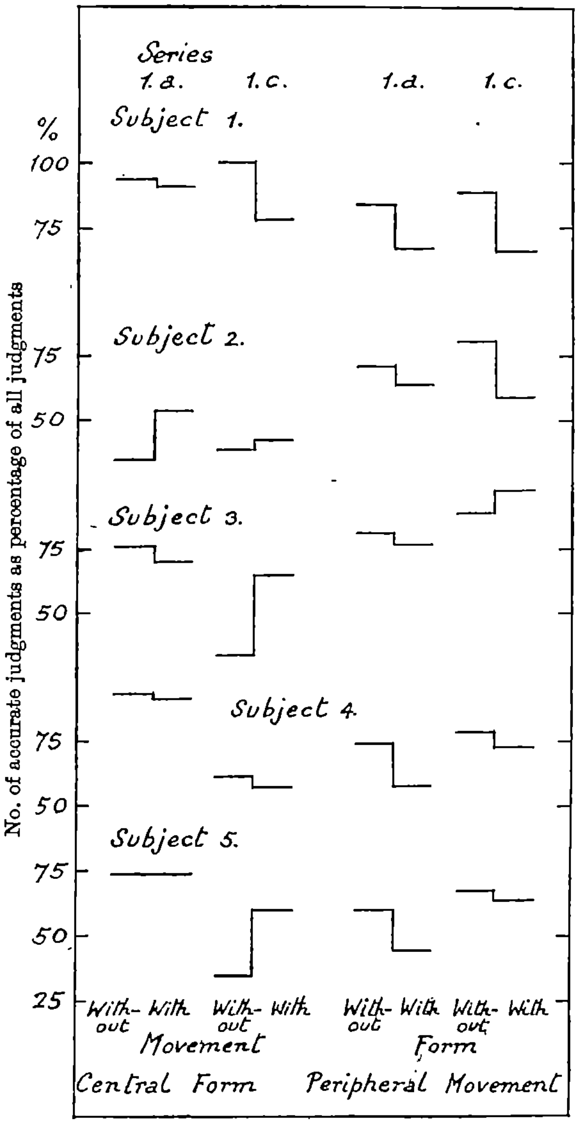

Fig. 3.

cases in which they did not perceive any movement was so small as not to give a fair measure.)

The effect of central form perception upon peripheral movement perception was quite different. In every case, except that of Subject 3 in Series 1 (*c*), fewer movements were observed when a central form was presented simultaneously than when there was no central form.

Quite clearly, then, when central forms and peripheral moving objects are presented simultaneously under these experimental conditions, it is in general the peripheral movement perception which is affected, rather than the central form perception. Nevertheless, one or two subjects reported occasionally that large movements in the periphery seemed to have a distinctly distracting effect upon central form perception. It was thought that, perhaps, if only a few large movements were presented to the subject, any effect produced by their perception might show up more clearly. Forms either the same as, or similar to, those given in Series 1 (*a*) and 1 (*c*) were presented with and without movements of 4 cm. in Series 1 (*b*) and 1 (*d*); and the results are shown in Table III. Clearly, the effect of simultaneous peripheral movement perception was, in general, to enhance central form perception. It appears probable that when no movement was seen, there was a state of expectation which interfered with central form perception much more

Table III. *Effect of peripheral perception of large movements upon central perception of form.*

	No. of accurate form judgments as percentage of all judgments					
	Series 1 (*b*)			Series 1 (*d*)		
Subject	No movement seen	Movement seen	Diff.	No Movement seen	Movement seen	Diff.
1	77	75	2	92	89	3
2	45	64	−19	75	92	−17
3	53	64	−11	50	75	−25
4	79	86	− 7	74	80	− 6
5	55	62	− 7	58	58	0

Table IV. *Effect upon central form perception of an object in the periphery.*

	No. of accurate form judgments in Series 1 (*c*) as percentage of all judgments		
Subject	With line	Without line	Diff.
1	88	100	12
2	41	63	22
3	44	56	12
4	59	56	− 3
5	47	75	28

effectively than did the actual perception of movement. If this state of expectation was removed by instructing the subject beforehand that no object would appear at all in the periphery, the number of central forms perceived correctly increased appreciably, as may be seen from Table IV, with the forms of Series 1 (*c*). That is to say, the conscious attitude of 'being on the look-out for a movement' definitely impaired the central form perception. But once the subject was actually 'on the look-out'

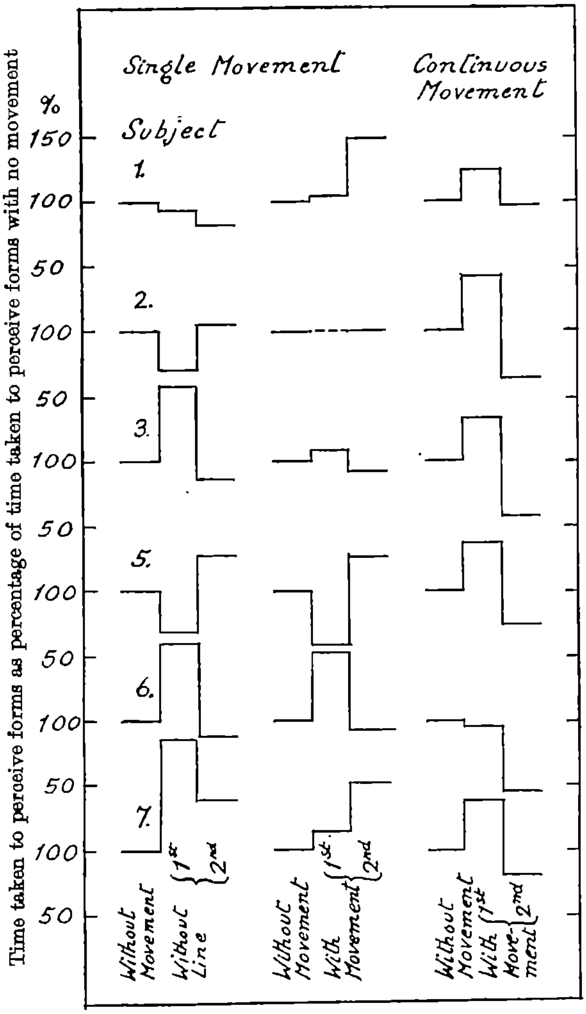

Fig. 4.

for a movement, either it made no difference to the form perception whether he saw the movement or not; or else seeing the movement actually improved central form perception by removing the conflicting attitude of 'being on the look-out for a movement.'

Other such psychic factors may be adduced to explain the lack of interference of peripheral movement perception with central form perception. These will be described later, on p. 224; but we must first deal with the following retinal factor. When the central form was exposed for a short time only, the excitation was at its maximum for an appreciable fraction of the exposure, before sinking to its lower, steady state. Now it was shown above, on p. 210, that the effect produced by a moving stimulus was partly due to the fact that it aroused a maximum excitation throughout the movement, and thus interfered more definitely with form perception when the excitation aroused by the latter had diminished, as it would during the course of a relatively long exposure. Therefore in Series 5 (*a*) the form was exposed for a period of one second or more, according as to how long the subject took to locate the square within the complex figure. And the movement of the peripheral object was not presented at the very beginning of the exposure. The first half of Table V and also Fig. 4 show the total time taken to perceive the square when no peripheral object was exposed, and when the peripheral line made a single movement, both expressed as percentages of the time taken to perceive the square when the peripheral line was stationary. It will be seen that the results are very irregular. Occasionally the per-

Table V. *Effect of peripheral movement perception upon central form perception during a long exposure.*

	With single movement								With continuous movement			
	Time taken to perceive forms with no peripheral object as percentage of time with stationary object				Time taken to perceive forms with moving peripheral object as percentage of time with stationary object				Time taken to perceive forms with moving peripheral object as percentage of time with stationary object			
	1st experiment		2nd experiment		1st experiment		2nd experiment		1st experiment		2nd experiment	
Subject	%	Diff.	%	Diff.	%	Diff.	%	Diff.	%	Diff.	%	Diff.
1	94	- 6	82	-18	102	2	149	49	123	23	98	- 2
2	71	-29	105	5	—	—	100	0	142	42	66	-34
3	159	59	88	-12	109	9	92	- 8	132	32	58	-42
5	69	-31	128	28	59	-41	128	28	138	38	73	-27
6	160	60	89	-11	154	54	94	- 6	97	- 3	45	-55
7	185	85	139	39	114	14	151	51	139	39	81	-19

ception time was greater when the peripheral line moved than when it remained stationary; but sometimes also it was greater when no peripheral object appeared at all. But the subject's introspections showed that, in certain cases, at the beginning of the experiments, the movement perception caused considerable distraction; it occupied the centre of consciousness, excluding the form perception, both during the actual movement, and for a considerable period afterwards. But the subjects reported that they very soon became accustomed to this new experimental method. Hence the initial advantage of the form perceptions with the stationary line is swamped in the general results.

In contrast to the earlier results shown in Table IV, no advantage is here observed for form perception in the absence of any peripheral object. This difference may be accounted for as follows. In Series 1 (*c*) the forms which were presented without any peripheral line made up a separate group, and the subjects were instructed beforehand that no line would appear in the periphery. In Series 5 (*a*) these presentations were scattered among the others, and no instructions of that kind were given. Thus the subjects were 'on the look-out' for a peripheral object which might at any moment appear and move. Consequently this state of expectation operated throughout to prevent the whole attention being given to the central form perception; and the time taken for this perception was equally slow and irregular whether the line appeared or not, and whether it moved or not.

In the second part of Table V and in Fig. 4 are shown the total times taken to perceive the triangle when the peripheral line was made to move continuously, expressed as percentages of the total times when the line remained stationary. It was thought that the effect of this continuous movement might be more marked than that of the single movement, since it occupied a larger fraction of the total time of exposure. During the first experiment with each subject the times for central form perception were in general considerably longer when the peripheral line moved than when it remained stationary; but this effect had in all cases disappeared by the second experiment. Clearly, then, the continuous movement, to which the subjects were unaccustomed, did at first exert a considerable effect upon the central form perception. But the mere fact of its frequent repetition set up a rapid process of habituation such that the subjects became completely accustomed to it. And in the second experiments, central form perception was more rapid when the movement did occur; for the movement perception then removed the uncertain state of 'being on the look-out for a movement' which was liable to operate when no movement was seen, and to impair central form perception.

Thus this series of experiments shows that central form perception is adversely affected by the appearance of something unusual or unexpected in the periphery. The subject, however, rapidly becomes accustomed to this appearance if it is frequently repeated; and then assumes an attitude of 'being on the look-out for something in the periphery.' This attitude in itself interferes with central form perception; this is shown by the two following observations:

(1) There is no improvement of central form perception when no peripheral object is presented, unless the subject has been informed of this beforehand, and has in consequence not assumed this attitude.

(2) When there is clear or continuous movement in the periphery which removes the attitude of uncertainty, central form perception may be more accurate than when the peripheral object remains stationary.

Undoubtedly the effect of this attitude can be attributed to the division and distribution of the attention, which must now cover the whole visual field, instead of the centre alone. This finding agrees well with the observations of Grindley ((8), p. 47) that "the simultaneous exposure of other forms at other parts of the visual field decreases the probability that any given form will be accurately reported."

(2) *Central and peripheral movement perception.*

It should be noted at the outset of this section that, in order to make some sort of correction for the relative phenomenal brightnesses of central and peripheral objects, the central line was made much smaller and narrower than the peripheral one. Unfortunately, owing to its narrowness, it now appeared so dim that it was often not observed by some of the subjects during long movements when it was moving

Table VI. *Effects of central and peripheral movement perception upon each other.*

	No. of correct movement judgments as percentage of all judgments					
	Central movement			Peripheral movement		
	Peripheral movement			Central movement		
Subject	Not seen	Seen	Diff.	Not seen	Seen	Diff.
1	50	57	− 7	84	64	20
2	50	59	− 9	71	65	6
3	56	44	12	81	65	16
5	47	56	− 9	60	68	− 8

throughout the exposure. Thus no conclusions can be drawn as to the comparative visibility of the central and peripheral movements; but the effects of each movement percept upon the other can be legitimately compared. This is done in Table VI and Fig. 5 for each of the four

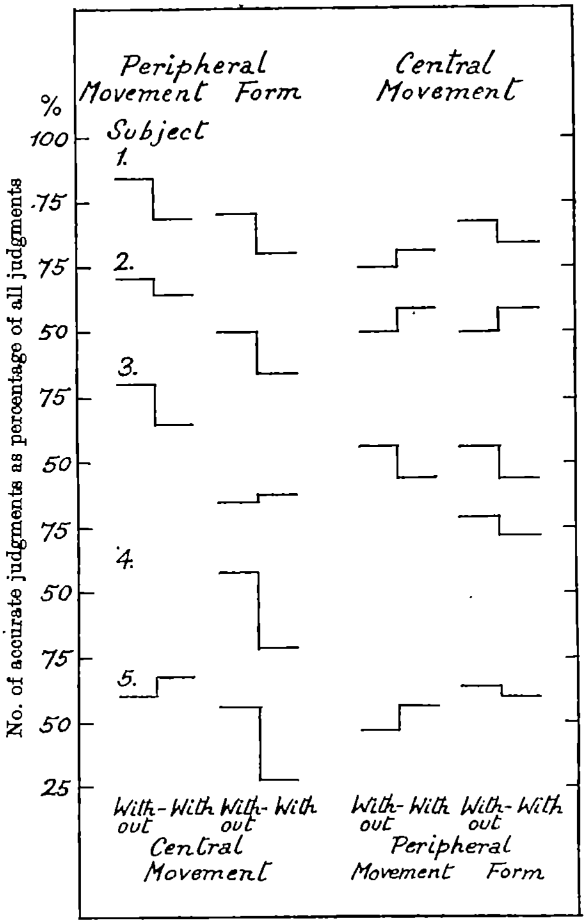

Fig. 5.

subjects separately. Except for Subject 5, the effect upon the peripheral movement perception of simultaneous central movement perception was almost as great as that produced by simultaneous central form perception (see Table II and Fig. 3). Very little effect was produced upon the central movement perception by the simultaneous peripheral movement perception; if anything, there was an improvement of central movement perception with simultaneous peripheral movement perception, just as there was some improvement of central form perception with simultaneous peripheral movement perception.

(3) *Peripheral form and central movement perception.*

It is interesting to compare the above results with the effects of peripheral form perception upon central movement perception. It is of course well known that it is impossible to perceive small complex objects in the periphery. Consequently large simple outline forms were employed, of approximately the same dimensions and brightness as the peripheral line in the other series. The forms were graded in size, but unfortunately, owing to a faulty arrangement of the series, only the smallest of the three sizes gave comparable 'movement' and 'no movement' judgments (this mistake was not repeated in Series 4). The numbers upon which are based Table VII and Fig. 5 are thus rather small. But they show that the perception of both central movement and peripheral form were in general impaired by simultaneous presentation. The peripheral form perception showed a percentage deterioration greater than that produced in peripheral movement perception by simultaneous central form or movement perception. But the effect upon central movement perception was also larger and more consistent for all subjects than was the effect produced by peripheral movement perception. That is to say, peripheral form perception, admittedly a somewhat imperfect function of the peripheral retina, interfered to a greater extent with central movement perception than did peripheral movement perception.

Table VII. *Effects of central movement perception and peripheral form perception upon each other.*

	No. of accurate judgments as percentage of all judgments					
	Peripheral form			Central movement		
Subject	No movement seen	Movement seen	Diff.	Without form	With form	Diff.
1	71	56	15	68	59	9
2	50	35	15	50	59	- 9
3	35	38	- 3	56	44	12
4	58	29	29	79	72	7
5	56	28	28	64	60	4

The non-interference of peripheral movement perception with central form perception, and the interference of peripheral form perception with central movement perception, when the latter was unaffected by peripheral movement perception, can be accounted for in several ways. It has been shown that the apparent non-interference and even enhancement of central form perception by peripheral movement perception was a result of the rapid habituation of the subject to the movement perception, and of his adoption of an attitude of expectation and divided attention. But it seems that peripheral movement and form perception have a differentiated effect upon the division of attention. Apparently the subjects, though rapidly becoming habituated to peripheral movement perception, were strongly aware throughout of the peripheral form. Thus there seem to have been certain factors at work which favoured form perception, as such, at the expense of movement perception, retaining the former at the centre of consciousness, whether or not the form was exposed at the centre or the periphery of the visual field.

The introspections of the subjects show that these factors were: (1) the difficulty of perception and apprehension of the objects presented, (2) the interest aroused by the objects. It was, of course, necessary throughout the experiments to make the forms rather difficult to apprehend; had they been completely cognized immediately, in all circumstances, it would have been impossible to demonstrate any interference with their perception by simultaneous movement perception. Even when 'attributively clear' (*i.e.* bright and easily visible), they were 'cognitively unclear' (*i.e.* it was difficult to make them out and understand their meaning, form relationships, etc.) (Dallenbach (4)). This difficulty aroused in the subject an effort at apprehension which served to keep the form in nearly every case at the centre of his consciousness, whether or not it appeared at the centre of his visual field. The effect of this difficulty upon simultaneous peripheral movement perception is shown by the fact that the number of peripheral movements perceived was smaller when the number of errors in central form perception was greater, *i.e.* when

Table VIII. *Effect of difficulty of perception of central form upon peripheral movement perception.*

	No. of accurate judgments as percentage of all judgments					
	Central form			Peripheral movement		
Subject	Group 1	Group 2	Diff.	Group 1	Group 2	Diff.
1	94	90	4	76	68	8
2	70	35	35	76	59	17
3	89	54	35	85	75	10
4	96	90	6	61	63	− 2
5	70	79	− 9	43	48	− 5

the difficulty of form perception was greater. In Table VIII the forms in Series 1 (*a*) were arranged in two groups, the first containing the rather simpler, the second the rather more complex forms. The number of correct judgments was greater in the first group both for central form and peripheral movement for Subjects 1, 2 and 3; while the number of correct judgments was less in the first group both for central form and peripheral movement for Subject 5. Thus in general the more difficult the subject found the forms the more mistakes he made in his perception of movement. Any difficulty in perceiving the movements, on the other hand, appeared to operate in the direction of increasing the interfering effect of the 'no movement' judgments (see Subject 2, p. 215).

Closely bound up with the factor of difficulty of apprehension was the second factor of interest. The subjects very rapidly lost all interest in the appearances of the moving line. On the other hand, it was of course necessary to change the forms frequently (since once fully cognized they were easily recognized even when only partly perceived). Consequently they always aroused a certain amount of interest. Some attempt was made to obviate this difficulty by substituting the pattern forms with a single variation employed in Series 1 (*c*) and 1 (*d*) for the varied forms presented in Series 1 (*a*) and 1 (*b*). A comparison of the results for Series 1 (*a*) and 1 (*c*) in Table II, and of Series 1 (*b*) and 1 (*d*) in Table III, will show that if the effect of the central interest factor was reduced, it was compensated for by the effect of the greater difficulty of apprehension of these pattern forms.

(4) *Peripheral form and movement perception.*

One more series of experiments was designed to test the relative dominance of form and movement perception in the periphery. In so far as movement perception is a more important and a more adequately performed function of the peripheral retina than is form perception, it might be concluded that if stationary forms and moving objects were presented together in the periphery, the perception of the former would be greatly impaired, while perception of the latter would be at the focus of consciousness, and thus would be unaffected. In Series 4 (*a*) the forms used in Series 3 were presented peripherally, together with the moving line which appeared rather further out in the periphery. It is seen from Table IX and Fig. 6 that in general both form and movement perception were less accurate when taking place simultaneously than when carried on separately. Since the percentages of forms perceived correctly were much less throughout than the percentages of movements perceived correctly, a better comparison of the effects of form and movement perception upon each other is afforded by considering the number of forms and movements perceived correctly when presented simultaneously,

Table IX. *Effects of peripheral form and movement perception upon each other (form and movement on same side of field).*

	No. of accurate judgments as percentage of all judgments						No. of accurate form judgments with movement as percentage of no. without movement		No. of accurate movement judgments with form as percentage of no. without form	
	Form			Movement						
Subject	No movement seen	Movement seen	Diff.	Without form	With form	Diff.	%	Diff.	%	Diff.
2	36	28	8	77	69	8	78	-22	90	-10
3	33	22	11	92	79	13	67	-33	86	-14
6	35	24	11	95	35	60	69	-31	37	-63
7	22	28	- 6	88	63	25	128	28	72	-28

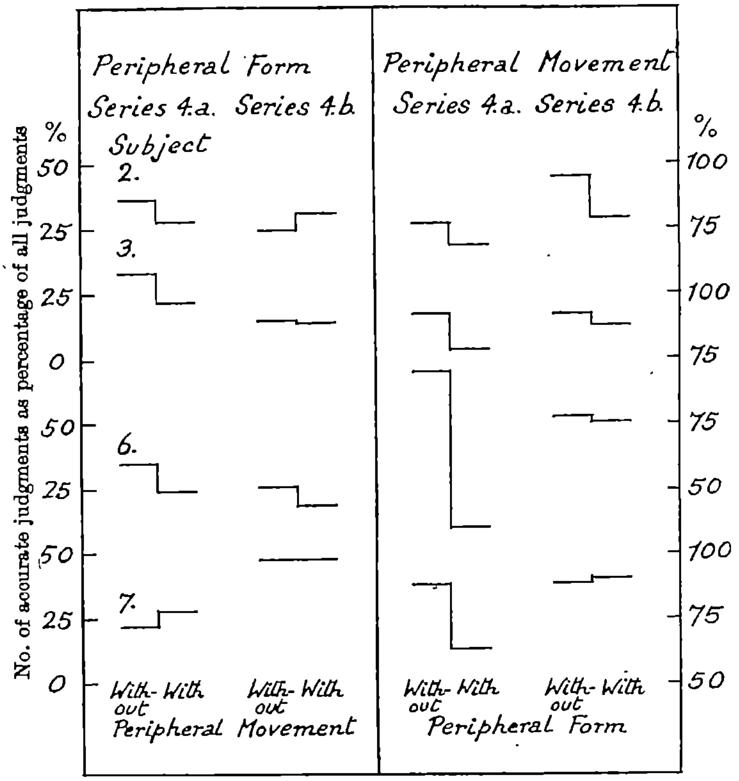

Fig. 6

expressed as percentages of the numbers perceived correctly when presented separately. This is done in the second half of Table IX. We see that for two subjects the percentage reduction of accuracy was greater for movement perception than for form perception; while for the other two subjects the reverse was true. Thus, clearly, in some cases, when the attention is divided between two simultaneous peripheral presentations, it is the form percept which retains the centre of consciousness; while the movement percept is relegated to the fringe. The greater clearness and phenomenal intensity of the movement percept are overbalanced by the difficulty and interest factors which are the accompaniments of form perception whenever attention is directed towards it. (Under everyday conditions, of course, attention is not normally directed towards the peripheral form field.)

Though the subjects all reported that it was difficult to notice what happened to the line when the forms were also present, yet at the same time it was sometimes reported that perception of the movement definitely acted as a distraction from the perception of the form; and Table IX shows this effect clearly for three out of the four subjects. Thus we must conclude that the central position, as well as the nature of the object, is also a factor which favours dominance of any particular percept in consciousness. For form perception, when central, was unaffected by peripheral movement, but when peripheral was impaired; while it was impaired to a greater degree by central movement than by peripheral movement although the peripheral line was much larger and brighter than the central one (cf. Tables VII and IX).

Further evidence of the importance of the relative positions, as apart from the nature, of the presentations, is given by the results obtained in Series 4 (*b*). Here the forms were presented peripherally on one side of the field (*i.e.* to one eye), and the movements on the other (*i.e.* to the other eye). The peripheral angles and brightnesses of the forms and moving object were the same as in Series 4 (*a*), and the forms were similar, though not identical. Yet it is clear from Table X and Fig. 6 that there was very little interaction of any kind between form and movement perception. Any effect upon form perception was equivocal, while movement perception was almost unaffected except in one case. Thus apparently when the two objects were sufficiently separated in space, and were presented to opposite eyes, there was practically no inter-action of any kind between their percepts. Thus, the nearer together in space the visual objects, the more difficult it was to distribute the attention; neither percept completely dominated over the other in con-

Table X. *Effects of peripheral form and movement perception upon each other (form and movement on opposite sides of the field).*

	No. of accurate judgments as percentage of all judgments					
	Form			Movement		
Subject	No movement seen	Movement seen	Diff.	Without form	With form	Diff.
2	24	31	-7	77	79	- 2
3	14	15	-1	92	88	4
6	26	19	7	95	76	19
7	48	48	0	88	90	- 2

sciousness, nor could attention be equally divided between them. Hylan (10) considers that no such thing as an actual division of attention can occur; but various activities of consciousness alternate with each other with extreme rapidity. But it is difficult to explain in any other way the apparent lack of conflict between the two percepts on opposite sides of the visual field. The well-known phenomenon of retinal rivalry would seem to preclude the possibility of absolutely simultaneous perception of the two objects by the two eyes. But why should it be easier to switch the attention rapidly from one percept to the other when the two objects are far apart than when they are near together? The introspections of the subjects of these experiments throw no light on this dilemma, which must therefore remain unsolved.

IV. Discussion and conclusions.

We must conclude from the foregoing data that the issue of the conflict between central form and peripheral movement perception is ultimately determined by the nature of these percepts. It was stated at the outset that movement perception possessed two advantages over form perception:

(1) The nervous excitation produced by a moving object tends to be at a maximum, while that produced by a stationary object of equal intensity will sink to a lower, steady level.

(2) In the peripheral retina the excitation produced by a moving object is enhanced through the process of summation resulting from the synaptic convergence of the neurones from the receptors. On the other hand, visual acuity, and hence accuracy of form perception, is greatly reduced in the periphery as a result of summation, and of the sparcity of receptors, and of distortion by the lens.

In addition, a movement percept, and especially a peripheral movement percept, may in normal circumstances be sudden and unexpected, emotionally toned and hence inherently interesting to the subject. Thus in everyday life the movement percept is likely to drive the form percept from the centre of consciousness.

But it was shown in these experiments that the interest factor connected with the movement percept rapidly disappeared. Introspective evidence of the disturbing and distracting effect of certain movements was given by some subjects. But all reported that this unexpectedness soon disappeared, and it seems that an attitude of divided attention was assumed, which has been designated as 'being on the look-out for something in the periphery'—or perhaps more correctly, as 'being on the look-out for a familiar presentation in the periphery.' This familiarity bred, if not contempt, at least a lack of interest, and a habitual and unchanging but non-focal type of awareness. Several subjects reported that they noted the movements of the peripheral object almost automatically. This agrees with the statements of Meisenheimer (11) that all phenomena make their first appearance at the centre of experience and develop there; but afterwards they may continue to exist outside the focus of experience in a wider field of consciousness, and remain there until attention is again turned upon them, attention being primarily the experience of turning-impulsively-upon. The retention of the focal position by the form percept, on the other hand, was much assisted by its central position in the field of vision. And, apart from this, the factors of difficulty of apprehension and of interest prevented the perception of form from becoming non-focal and semi-automatic.

Thus it seems that physiologically the peripheral retina is well adapted for the perception of movement. Psychologically also the movement percept under normal conditions seems to be accompanied by interest and affective factors which ensure its appearance at the centre of consciousness—always provided that the individual has not acquired a habitual semi-automatic type of response, as under the conditions of these experiments. We see, then, that Exner (5) was to some extent justified in regarding the peripheral retina as an organ specially adapted for movement perception.

But the theory has been advanced by Parsons (12) that this adaptation is structural rather than functional. He considers that the photopic and scotopic mechanisms of vision may correspond to the epicritic and protopathic or dyscritic mechanisms which were postulated by Head (9) for cutaneous sensibility. Parsons points out that "scotopic vision shows many of the characteristics of protopathic sensibility, especially in its vagueness and crudity, its undifferentiated character and lack of exact localization and discrimination" ((12), p. 176). On the other hand, this mechanism has acquired certain powers of discrimination and adaptability (as shown in dark adaptation), and no longer shows any all-or-none reactions. Moreover, we are uncertain as to the exact behaviour of the scotopic mechanism at any but very low intensities of illumination. For although the fovea contains only the photopic receptors, the cones, the peripheral retina contains both rods and cones, according to Chievitz (8) in a proportion of one cone to 15 to 18 rods all over the peripheral retina outside the area centralis. Thus the behaviour of the peripheral retina at fairly high intensities of illumination may well be a function of the cones, such as they are, and not of the rods at all. Parsons indeed points out that "there is overwhelming proof...that peripheral vision behaves in exactly the same manner as central vision, but with diminished sensitivity" ((12), p. 177). But the apparently inherent sensitivity of the peripheral retina to moving objects is advanced by him as evidence of its dyscritic nature. It is known that the vision of the lower animals, which presumably is dyscritic in sensibility, is particularly sensitive to movement; hence it is concluded that the region of the retina most sensitive to movement is also dyscritic in origin. Evidence in favour of this view is given by the work of Riddoch (13), who found that in cases of gradual recovery from occipital injuries, the first visual stimuli perceived were light and movement; and that recovery of vision for movement began in the peripheral field. It is, of course, possible that under such conditions only the scotopic mechanism would be functioning, and that it was indeed demonstrating its dyscritic nature. Again, in the normal individual an epicritic mechanism usually dominates over a dyscritic mechanism—that is to say, the dominance of the central form percept over the peripheral movement would on this theory be expected. But if at all but very low intensities of illumination peripheral movement perception is carried on by the cones rather than by the rods, the experiments described in this paper have shown merely that one part of the epicritic mechanism will at one time dominate over another, while at another time the dominance is reversed.

Moreover, we have adduced much evidence to show that the relative sensitivity of the retina to form and movement perception is greatly influenced by the psychic factors of interest, familiarity and difficulty of apprehension. It is the relative strength of these various factors as they function in connection with the different percepts that determines which of these percepts shall be at the focus of consciousness at any given moment, and which at the fringe. This psychic and functional method of determination appears to us to afford a better explanation of the known facts than a structural determination based upon the theory of epicritic and dyscritic sensibility.

V. SUMMARY.

1. To throw some light upon the question as to whether the peripheral retina is inherently adapted to the perception of movement, a series of experiments was carried out to study the nature of simultaneous form and movement perception in the central and peripheral retina.

2. It was found that the accuracy of movement perception, both central and peripheral, was almost always impaired by simultaneous perception of a stationary form, whether peripheral or central. The accuracy of peripheral movement perception was also decreased by simultaneous central movement perception, but that of central movement perception was unaffected by simultaneous peripheral movement perception.

3. The accuracy of central form perception was slightly decreased whether the peripheral object moved or remained stationary. But the accuracy of peripheral form perception was greatly decreased by simultaneous perception of movement, whether central or peripheral.

4. It appeared that the subjects of these experiments adopted an attitude of distributed attention, designated as 'being on the look-out' for various objects in the visual field. But there appeared to be tendencies for consciousness to centre upon: (*a*) the form, partly because it was more difficult to apprehend than the movement, and partly because it was more interesting; (*b*) the movement, momentarily, at the expense of the form, when it was unfamiliar or unexpected; (*c*) the object at the centre of the visual field, whatever its nature, because it is more customary to concentrate on foveal than on peripheral percepts.

5. When the form and the movement were situated close together in the periphery, there was much more conflict and fluctuation of attention between them than when they were situated on opposite sides of the field of vision.

6. There was no indication of any special sensitivity of the peripheral retina to movement; its reactions appeared to be determined principally in accordance with the three tendencies described in 4 (*a*), (*b*) and (*c*).

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VISUAL ACUITY AND ITS PHYSIOLOGICAL BASIS.

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I. *Introduction* (pp. 233–4).
II. *Definition and measurement of visual acuity* (pp. 234–7).
III. Minimum separabile *and* minimum visibile (pp. 237–8).
IV. *Visual acuity and the retinal mosaic* (pp. 238–45).
V. *Visual acuity and* Raumsinn (pp. 245–7).
VI. *Visual acuity and illumination* (pp. 247–9).
VII. *Hecht's theory of the relation between acuity and illumination* (pp. 249–58).
VIII. *Summary* (p. 258).
References (pp. 259–61).

I. INTRODUCTION.

IT is a familiar fact that visual acuity, or the resolving power of the eye, varies with illumination. This fact has long presented an important problem for physiological optics, but in recent times the problem has assumed especial interest on account of the theoretical work of Selig Hecht (24). Hecht has developed a theory which not only attempts to account for the relation between visual acuity and illumination in exact quantitative terms, but which also has far-reaching implications for the general theory of vision, and, indeed, for sensory physiology in general.

The work of Hecht has stimulated the present writers to re-examine the problem of visual acuity, both theoretically and experimentally. Although we are here primarily concerned with the special question of the relation between visual acuity and illumination, it seems very desirable to begin with a preliminary discussion which shall clarify our general ideas regarding visual acuity, and make plain the theoretical problems at issue.

We shall first consider how visual acuity is to be defined, and second, we shall examine, in the light of experimental knowledge, the general problem of the physiological basis of this function. We shall then review the theories concerning the dependence of acuity upon illumination, with particular attention to the theory of Hecht. We are led, in fact, to the conclusion that this theory, despite its attractive simplicity, is based on unsound assumptions, and that its quantitative adequacy to the facts is more apparent than real.

If Hecht's theory cannot be admitted, it still remains to inquire what actually causes acuity to change with intensity. Experiments carried out by Wilcox attempt to answer this question. In the course of these experiments a new hypothesis was developed which, fortunately, could be quantitatively formulated and put to experimental test. The hypothesis is, in fact, found to be confirmed. There is thus direct evidence that the true explanation of the relation between visual acuity and illumination lies in quite a different direction from that proposed by Hecht. These experiments, and their theoretical implications, will be discussed in a second paper[1].

II. DEFINITION AND MEASUREMENT OF VISUAL ACUITY.

Visual acuity, or the resolving power of the eye, is defined by Hofmann ((31), p. 19) as the capacity to perceive fine details. Other writers have proposed different definitions; but there is fairly general agreement regarding the method by which visual acuity is actually to be measured. Such a measurement is taken to consist in a determination of the smallest interspace, in terms of visual angle or retinal distance, at which two neighbouring objects can be resolved, *i.e.* perceived as two rather than as one. The acuity is then inversely proportional to this minimal angle. The ophthalmologist uses letters or numbers as his test objects, but it is generally acknowledged that the recognition of such objects is a complex function, not amenable to exact analysis or measurement.

If, as is frequently done, we choose to employ the term 'visual acuity' in its most inclusive sense, we can distinguish three different sorts of visual acuity. These may best be described in terms of the tests by which they are measured.

(1) The first type of acuity is the resolving power, or visual acuity in the narrow sense. The test object may consist simply of two neighbouring dots. The measure of the acuity is the reciprocal of the least visual angle at which the two points can be perceived as separate. Or, instead of points, one may use two parallel lines side by side. Often, in place of two lines, a large group of parallel lines, forming a grating, is used as test object. The square letter-E figure ('hook') of Snellen, and the Landolt

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[1] A brief account of these experiments has already appeared elsewhere (57).

'ring,' or 'broken circle,' are generally considered as measuring essentially the same visual function[1].

In all these tests, we note, the observer's task is the perception of a gap (or a series of such gaps) between two contours which are side by side. Usually, however, a distinction is made between the resolution of *two objects* and the perception of a *single* small object. A measurement of the visual angle subtended by the smallest object (point or line) that can be distinguished from its background is not ordinarily regarded as a test of 'visual acuity.' But certain writers—Guillery (21), Hofmann (31), Pergens (46) and Koster (38)—consider that there is no fundamental difference between these two situations. As we shall see, this is a question of much theoretical importance.

For the sake of convenience, we shall speak of the least visual angle for the separation of parallel lines, Snellen hooks, and the other conventional acuity test objects, as the 'resolution threshold.' With good illumination, this threshold is generally found to range between 30″ and 60″ for the normal eye.

(2) It was first shown by Volkmann (55) that the separation threshold does not represent the ultimate limit of the eye's power of spatial discrimination, but that very much lower values may be obtained when this power is tested in a different manner. Volkmann measured the threshold for the perception of a difference between the *breadths* of two simultaneously presented lines, and obtained strikingly low values, ranging from 7″ to 11″.

Values of the same or even a lower order of magnitude are obtained when one measures the so-called 'vernier threshold' or 'displacement threshold,' *i.e.* the threshold for the perception of a lack of alignment between two straight lines placed end to end. This was first discovered by Wülfing (59). French (18) finds that the average error of an alignment setting may be as low as one second of arc.

The capacity to detect a slight displacement of a single *moving* object is not usually considered as falling under the strict definition of visual acuity. However, it is obvious that spatial discrimination is involved in this perception, and the movement threshold is of much interest in connection with the general theory of visual acuity. It is significant that the thresholds obtained are of the same order of magnitude as the values for the vernier threshold (Volkmann (55)). In fact, Stratton (54) found a very close agreement between the two.

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[1] In Snellen's test one determines the greatest distance at which the figure can be resolved, as indicated by a correct judgment regarding the orientation of the 'E,' which the observer does not know in advance and which is changed from trial to trial. In Landolt's test, the observer must report the position of a small open segment on the circumference of the ring. (This test has been objected to, on the ground that it involves only the perception of a single object, and not the separation of two objects.)

A good deal of theoretical discussion has been devoted to the problem of explaining the very low thresholds which are obtained under these various special conditions, as contrasted with the values obtained for the ordinary resolution threshold. In the early period of physiological optics, it was generally taken for granted that the limit of the eye's delicacy of spatial discrimination could be measured in terms of the least angle permitting discrete perception of two contours situated side by side. But Hering (29) was led, as a result of Wülfing's experiments, to conclude that one must draw a distinction between the resolving power (*Auflösungsvermögen*) or ordinary visual acuity, and the space sense (*Raumsinn*) or power of distinguishing differences in spatial position. According to him, a measure of the *Raumsinn* is furnished by the vernier acuity; the older tests, which measure only the *Auflösungsvermögen*, do not enable us to reach the true limit of spatial perception. The underlying theory has been most adequately worked out by Anderson and Weymouth (3) (*vide infra*).

(3) Still another sort of acuity is the stereoscopic acuity, or fineness of binocular depth perception. Since the perception of depth is a response to the disparity, or difference in relative retinal location, between two monocular patterns, this acuity might seem to correspond exactly to the acuity of type 1, and one might expect to find a corresponding value for the threshold. It is experimentally found, however, that the stereoscopic threshold, *i.e.* the minimal angular disparity which will give rise to a perceived depth difference, is much lower than the resolution threshold. The threshold proves to be of the same low order as the vernier threshold (Bourdon (9)). Howard (35), Anderson and Weymouth (3), and Langlands (42) have obtained depth thresholds corresponding to a disparity as low as 2″.

Here, again, the discrepancy with the threshold for monocular separation of contours has given rise to much perplexity. Helmholtz, for example, had taken it for granted that the just perceptible difference in depth could be computed from the resolving power for parallel lines. As in the case of the vernier threshold, various speculations have been made in the effort to reconcile the apparent contradiction. Stratton (53) made the astute suggestion that the stereoscopic acuity, from the theoretical point of view, is comparable to the vernier acuity rather than to the ordinary resolving power. The same standpoint is adopted in the recent theoretical work of Anderson and Weymouth (3).

The present study is primarily concerned with visual acuity in the narrow sense (visual acuity of type 1). The acuities of types 2 and 3 are of interest, however, in connection with the general theory, and we shall have some occasion to refer to them in the course of our discussion.

III. *MINIMUM SEPARABILE* AND *MINIMUM VISIBILE*.

We return now to the problem of the resolution threshold (visual acuity of type 1). Although certain statements in the literature might lead one to suppose that a certain definite standard value might be assigned to this threshold, this would be a very mistaken conception. Even for one and the same individual, the threshold obtained depends upon the particular type of test object employed (as well as upon the illumination, and the brightness difference between test object and background). Thus, if one makes tests with a series of objects varying in size and form, one obtains a wide range of different values for the threshold. This is very graphically demonstrated by Guillery ((21), p. 799 and Fig. 247).

Even in the simple case of two parallel lines, we find that the acuity varies, in a pronounced way, with the *width* of the lines, and with their *length* (4, 48). For example, Roelofs (48) obtained a resolution threshold of 89″ in the case of two lines each 18″ wide and 89″ long. On the other hand, when the width was 66″ and the length 11^1, the threshold was only 22″.

It has been customary to draw a sharp distinction between the *minimum separabile*, illustrated by the threshold for discrete perception of two lines, and the *minimum visibile*, which would be exemplified by the least visual angle at which a *single* line could be recognized as distinct from its background. But how is one to make a clear differentiation between these two sorts of threshold? The following consideration will show the difficulty of this. Suppose, starting with two fine parallel lines, we gradually increase their width. The resolution threshold will decrease continually. Eventually, we reach the conditions corresponding to the *minimum visibile*; and, let it be noted, the final threshold which we obtain will be very small as compared with the threshold found under the initial conditions[1].

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[1] In the case of a single bright line on a dark background, the width may indeed be as small as one pleases and yet the line will be seen provided the intensity is sufficiently high—just as we see the stars despite the infinitesimal visual angle which they subtend. In actual experiment, Bell (7) obtained a threshold of 0·5″ for the perception of a single bright line,

At what width of the lines do we pass from the *minimum visibile* to the *minimum separabile*? One finds no definite answer to this question even in the writings of those who insist upon the importance of the distinction, and who hold that the *minimum visibile* does not furnish a true measure of 'visual acuity.' For the sake of clarity, one might decide to define the *minimum separabile* (in the case of two parallel lines) as the least perceptible separation between two stimulus-lines of infinitesimal width (in terms of visual angle). If, on the other hand, one had two surfaces with adjoining parallel rectilinear contours, the breadth of the surfaces being so great that a further increase in breadth would not improve the discrimination of the gap between them, the threshold of resolution in this case might be defined as the *minimum visibile*. (These definitions, besides being formulated for the special case of parallel lines, would be still further relative in that the *length* of the lines introduces an additional variable.)

The customary test objects used in the measurement of 'visual acuity' contain lines of very appreciable though small width; hence, in the sense of the above definitions, the threshold obtained is neither the *minimum separabile* nor the *minimum visibile*, but an intermediate value. Is this threshold a true measure of visual acuity?

The definitions we have given are arbitrary. But if one adheres to the prevailing belief that 'visual acuity' is a function essentially different from that involved in the discrimination of single lines or points, it is clear that some definite criterion is necessary, so that one may know exactly how this function is to be measured.

If, on the contrary, one holds with Guillery and Hofmann that there is no ultimate dissimilarity, as regards the visual functions involved, between the *minimum separabile* and the *minimum visibile*, this question of definition is no longer one of fundamental importance. Discussion of this point may best be postponed until we have considered the problem of the relation between acuity and the structure of the retina.

IV. VISUAL ACUITY AND THE RETINAL MOSAIC.

In 1866 Helmholtz ((27), pp. 215 ff.) pointed out that if the rods and cones are the photosensitive elements of the retina, the finite size of

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but there is no reason to doubt that this value could be reduced indefinitely by increasing the light intensity.

A single dark line on a bright ground can be seen even though its width is exceedingly small. Barnard (6), Lowell (43), Slipher and Lampland (62), and Pickering (47) have all obtained a *minimum visibile* which is less than 1″ in this case.

these elements should place a certain limitation on the resolution of extremely fine patterns. If we imagine the fovea to be stimulated, *e.g.* by two mathematical point-images, it is obvious that a perception of two discrete points will be impossible unless the images are separated by at least one cone diameter. For if this is not the case, then the two images will fall on the same cone, and necessarily give rise to a unitary perception; or they will fall on adjacent cones, and there will be no basis for the perception of a gap between them. A similar conclusion can be drawn in the case in which the retinal image consists of two lines of infinite thinness.

Helmholtz's guiding idea is expressed in the following words: "The light which falls upon a single sensitive element can produce only a single light sensation, within which it is impossible to distinguish whether individual parts of the element are strongly illuminated, others weakly illuminated" (27), p. 215). Thus he considered that the structure of the retina places an ideal limit to the resolution of fine patterns for the reason that it places a spatial limit to *brightness discrimination.*

Two aspects of Helmholtz's conception should be especially emphasized. If we test the eye's resolving power, employing as test object a pair of points or lines of infinitesimal surface, theory predicts that we cannot obtain a threshold which (expressed in terms of retinal distance) will be less than the cone diameter. We cannot, however, predict that the threshold will be equal to this value; for it may well be greater. In other words, the ideal limit which is established by retinal structure may not actually be reached. Whether or not it is reached can only be decided by actual test.

Furthermore, Helmholtz recognized that this theoretical principle, namely, that the resolution threshold cannot fall short of a value corresponding to a cone diameter, only applies to the special case of mathematical points or lines. When the test object consists of two or more lines *of finite width,* the threshold may, as he was well aware, be considerably less than this value.

Thus Helmholtz did not make the assumption that 'visual acuity' in general is determined simply by the structure of the retinal mosaic. Visual acuity depends primarily on brightness discrimination, and only secondarily on the dimensions of the cones. Moreover, the test objects which are conventionally used in the measurement of acuity are not mathematical points or lines; in such a case the visual resolution of adjoining contours may well surpass the fineness of the mosaic.

However, Helmholtz's clear analysis of the problem seems to have been frequently overlooked by subsequent writers. In much of the literature one finds emphasis laid on the correlation of the limits of acuity with retinal structure, without due regard to the precise nature of this correlation, and its limitations. Weber's (56) idea of 'sensation circles' is still widely current (though objections have been raised against exclusively anatomical interpretations of visual acuity by Hartridge (22) and by Dunlap (14)).

It appears to be universally recognized that the *minimum visibile* is not simply determined by retinal structure; the narrowest perceptible single line may subtend an angle much smaller than that corresponding to a cone diameter. The explanation, it is agreed, is to be found in the fact that the *minimum visibile* involves only the discrimination of a brightness between this single line and its surroundings.

But, we may ask, is any essentially different process involved in the discrete perception of two lines (*minimum separabile*)? For the perceptual task is essentially the same: to see two lines as distinct from each other really means to see a third line, namely, the line which forms the gap between the two. And if the perception of a single line rests on a discrimination of brightness, why should not the same be true of the resolution of a pair of lines? There is a difference, of course, in the extent of the 'background' contiguous to the line which is to be distinguished. There is also an enormous difference in the actual acuity of the eye as measured by the two methods. But there is no apparent reason for supposing that any essentially different visual function is involved in the *minimum separabile* as contrasted with the *minimum visibile*.

Let us consider in greater detail the relation of acuity to retinal structure, and let us begin with the case of the *minimum visibile*. Here our test object is a single line on a wide background of different brightness. It is evident that the perception of this line simply requires that there shall be at least one row of cones whose stimulation is sensibly different from that of the rows on either side. Even if the breadth of the retinal line-image is only a small fraction of a cone diameter, the cones upon which it falls will not receive the same quantity of light as the surrounding cones. Provided brightness discrimination is sufficiently delicate to respond to this small difference, a line will be seen.

Now, let us assume that the background on which our central line appears is narrowed down until it consists simply of two fine lines. We shall suppose that we have two bright lines which are separated by a dark space[1], and that the width of the schematic retinal image of each bright line is only a fraction of a cone diameter[1]. Then, if we consider the three cones in Fig. 1, we see that the left- and right-hand cones may receive a larger quantity of light than the central cone, even though the distance between the inner contours of the lines is less than one cone diameter. Provided this difference of illumination can be responded to, the space separating the bright lines will be perceived. Thus there is no *a priori* reason for supposing that the resolution threshold, even for lines of this minute width, may not have a lower value than would correspond to the diameter of a single cone.

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[1] A similar argument can be applied to two dark lines separated by a bright interspace.

It is obvious, to be sure, that our two lines cannot be brought indefinitely near to each other and still be resolved. As Helmholtz has noted, resolution is impossible when the *centres* of the two light-bands on the retina are separated by only one cone width (27), p. 218). Thus the resolving power is not *independent* of retinal structure. In virtue of the discrete structure of the receptor layer, a continuous distribution of light intensity on the retina is transformed, from the physiological point of view, into a multiplicity of discontinuous spots. This places a definite limitation upon the perception of brightness differences.

However, it is only when the two linear stimuli are infinitely narrow that the ideal resolution threshold becomes equal to the width of a cone. So long as they have any appreciable width, there remains the theoretical possibility of obtaining a threshold which is less than the cone diameter[2].

In the case pictured in Fig. 1, it is apparent that, as our two bright lines are moved nearer together, the quantity of light received by the left- and right-hand cones is diminished, while that received by the central cone is increased. When the distance between the inner contours of the lines is made sufficiently small, all three cones will be equally stimulated. Such a retinal distance will represent the ideal limit of acuity for this particular test object.

But it is evident that if the two images on the retina have a width as great as 1½ times the diameter of a cone, the quantity of light received by the two outer cones will not be altered by shortening the distance between contours. Thus, no matter how small this distance, the central cone will always receive less light than its neighbours on either side.

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[1] For our present purposes the aberrations of the eye, which cause the light to spread beyond the contours of the schematic image, may be neglected.

[2] In the interesting psychophysical study of visual acuity which has recently been made by Coover (12), it was found that the test object (Landolt ring, Ives grating, etc.) was frequently 'resolved' when the 'gap' (in terms of retinal distance) was less than the cone width. The author supposes that such judgments are necessarily based on 'secondary criteria.' We can see no theoretical justification for this assumption.

For lines of this width, or any greater width, the retinal mosaic prescribes no absolute limit to resolution.

We emphasize that by 'theoretical' or 'ideal' thresholds we refer to the thresholds which could conceivably be attained if the structure of the mosaic were the only factor limiting acuity. Actually, however, acuity is limited by other factors. Therefore, except possibly under certain special conditions, these ideal values are not attained in practice.

This is especially obvious when we consider the resolving power for luminous points. When the components of a double star are separated by as little as 3½′, only an observer of exceptionally keen vision is able to resolve them (Aubert (4), p. 203). Now, the diameter of a foveal cone, representing the 'ideal' threshold for the resolution of two points, corresponds to a visual angle of about 30″ (cf. (58)). Thus the actual performance in this case falls far short of the limit set by retinal structure.

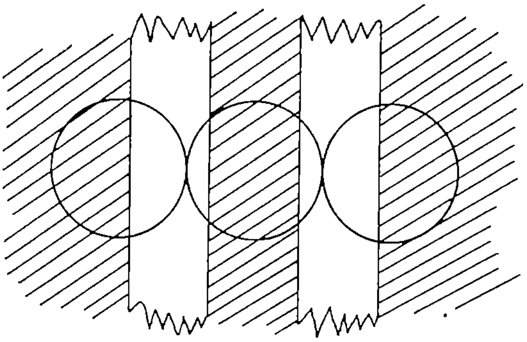

Fig. 1.

This should not surprise us, when we consider the fact that the eye is by no means a perfect optical instrument. Thus a luminous point in external space is not focused upon the retina as a mathematical point, but as a surface of very appreciable size, in which the intensity only gradually falls off towards the edges.

Taking account of chromatic aberration, diffraction, and other sources of optical error, Hartridge has performed calculations of the amount of this spreading. We quote this author: "Taking for example the case of a bright narrow line on a dark ground, and assuming the diameter of the pupil to be 3 mm. and that the diameter of the foveal cones is $3{\cdot}2\,\mu$. If then the intensity of illumination of that row of foveal cones which exactly corresponds with the centre of the image of the line equals 100, then the intensity of illumination of the rows of cones which lie on either side of the central row will be found by calculation to be 31 per cent.; and the next row of cones will be found to be illuminated by approximately 9 per cent., and so on. And it is not until the sixth row of cones is reached that the intensity of illumination has fallen as low as 1 per cent. In other words, even in the case of the image of a linear source, a large number of retinal cone elements receive stimulation, and it is not possible to think of any one cone or even any one small group of cones as being alone stimulated" ((22), p. 57).

Schoute (51) has made experimental measurements, by an objective method, of the 'diffusion circles' in the normal, well-accommodated eye. For a pupillary diameter of about 5 mm. he finds that the diffusion circles in central vision are from 160 to 220μ in diameter.

The existence of this large optical spreading can be demonstrated in a variety of ways through its effects on subjective phenomena. Thus, it is found that visual acuity is affected by the size of the pupillary aperture, even though the strength of the retinal illumination is kept constant (11). It is also found that visual acuity may be greatly improved by substituting monochromatic light for light which contains a mixture of wave-lengths; this improvement may amount to as much as 300 per cent. (44).

If we consider the large dioptric errors of the normal eye, then it becomes a surprising fact that the subjective blurring of our perceptual patterns is not even far greater than it actually is, and indeed that we see any definite and clear-cut contours in our visual field. But, as Mach (45) first suggested, these errors are probably compensated, in large measure, by the effects of *contrast*, or interaction of visual areas.

Hering has also laid great emphasis on the importance of contrast for sharp vision. According to him, the broad haze at the edges of an illuminated retinal region is neutralized (so far as sensation is concerned) by an antagonistic induction of black due to the higher brightness immediately adjacent. In his own words: "The retinal image of every contour line, even when seen with the best accommodation, is blurred. But our 'inner eye' possesses the power of creating in the psychical field of vision (provided the intensity of this imperfect line-image is sufficiently great) a sharp boundary between the two contiguous colours—thus of producing sharply contoured visual objects out of the diffusely contoured elements of the retinal image. Our visual organ owes this power to the interaction of visual areas. The retinal image is always blurred; but, like the photographer who retouches a defective print, this interaction corrects the picture of external objects. In those places where light is lost by aberration, interaction replaces this loss of brilliance more or less adequately; where excess light penetrates to disadvantage, interaction renders it ineffective by darkening" ((30), p. 154).

The resolution of an acuity test object, as we have seen, depends essentially on the perception of a brightness gradient. Optical blurring, due to the defects of the image-forming apparatus of the eye, tends to reduce the gradient on the retina and hence to diminish acuity. Simultaneous contrast, on the other hand, enhances the *perceptual* gradient, and thus makes up, in greater or less degree, for the effects of blurring.

In terms of this conception we can understand why acuity should vary so much with changes in the size and form of the test object. We know, for example, that contrast increases with the area of the contrast-inducing surface. If we have a single dark line on a large white field, the contrast-darkening will be much greater than in the case of a dark line lying between two narrow white lines. Thus the great difference in the corresponding values of the resolution threshold becomes intelligible[1].

In the case of a test object, consisting simply of two luminous points, the visual acuity is very low when considered in relation to the anatomical limit. With other test objects, offering more favourable conditions for simultaneous contrast, the eye's performance is considerably better. It is an interesting question whether, given optimal conditions in respect to type of test object and in respect to illumination, the anatomical limit of acuity can actually be attained.

Helmholtz found that he could resolve two parallel white lines, under the most favourable conditions, when the distance between their centres amounted to 64″ (27), p. 216). Since this angle corresponds to the diameter of the foveal cones, as measured by Kölliker, he concluded that "the optical structure of a well-built and well-accommodated eye is fully adequate to achieve the degree of precision which the size of the nervous elements makes possible." Later histological studies, however, have shown that Kölliker's values are far too large to represent the size of the cones in the centre of the average fovea. Thus Helmholtz's acuity was considerably below the anatomical limit for the given test object.

It is possible that, under still more favourable conditions, the acuity may reach the highest value which is compatible with the dimensions of the cones. But, so far as we are aware, there exist no data to prove that this is the case. Under the usual experimental conditions, at any rate, the acuity seems to be definitely inferior to the anatomical limit.

In any case, the highest acuities are obtained only under optimal conditions of illumination. The acuity is not merely a function of the size and form of the test object, but depends also on the brightness of the object and the brightness of the background on which the object appears.

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[1] For a discussion of other factors which enter into this problem, cf. Hofmann (31), p. 26).

This fact, again, indicates that the eye's acuity is primarily restricted by functional factors, rather than by the dimensions of the receptors.

V. VISUAL ACUITY AND *RAUMSINN*.

In the preceding section we have analysed the conditions pertaining to the perception of a gap between adjoining points or contours. We have seen that the visual acuity depends primarily on the perception of a brightness gradient. The structure of the retina places certain ideal limits to the perception of brightness gradients, and hence to the fineness of acuity; whether or not these limits are actually reached, they cannot at any rate be exceeded.

The fineness of visual discrimination may, however, be measured by other methods which involve rather different principles. For example, we may test an observer's ability to detect a slight relative displacement between two parallel contours placed end to end—the so-called vernier acuity.

Here the conditions are quite different from those appropriate to the perception of an interspace between the same two lines placed side by side. In the latter case resolution is possible only if, between the edges of the two retinal images, there lies a row of cones which are *sensibly different in respect of illumination* from the two rows corresponding to the contours. The perception of a lack of alignment between two lines placed endwise, on the other hand, does not demand that a *gap* shall be perceived between the two lines.

The vernier threshold proves to be exceedingly small. A retinal displacement amounting to 1/5 the diameter of a cone, or even less, can be detected under favourable circumstances.

At first thought, it might appear that no displacement ought to be seen until the shift on the retina amounted to at least a whole cone diameter. But Hering and Best have pointed out that, even though the retinal displacement is much smaller than this, it is possible for one contour to stimulate a row of cones, while the other contour fails to stimulate any cones lying in the same line (cf. (21), pp. 772 f.). Thus the 'physiological boundaries' of the two contours would not lie on the same row, but on adjacent rows, and the displacement could therefore be recognized.

According to this theory, the vernier threshold is a function of brightness discrimination; this discrimination, of course, being subject to a limitation which is set by the size and arrangement of the receptors. It is probable, however, that this theory is over-simplified, and that a complete theory will have to include other factors, such as are brought out by Weymouth and his collaborators (3, 5) in their theory of 'mean local sign.'

These writers point out, to begin with, that the retinal image of a straight contour will in general be received by an irregular and not a perfectly straight row of cones. Since we *see* a straight and not a broken line, a 'smoothing' or 'averaging' process must take place among the 'local signs' of the individual cones in the irregular row. The position of the perceptual straight line is determined by the 'mean local sign.'

If we take into account the continual movement of the eye, we must conceive this averaging process to be extended over a large number of such rows which successively receive the image. Hence the apparent position of the line will be based upon the mean of a very large number of individual 'local signs.'

This apparent position, being determined, not by a single element but by the co-operation of many, may be changed appreciably by a very minute displacement of the objective contour. The fineness of perception of displacement "is therefore not restricted to such units as interconal distance or cone diameter but may be accurate to a small fraction of these units just as the mean of a number of measurements made in inches may be accurate to a small fraction of an inch" ((3), p. 586).

The vernier threshold may therefore be much smaller, in terms of retinal distance, than the cone diameter—how much smaller, we cannot predict. The authors have applied the same theory to the stereoscopic acuity. They find it confirmed by the following facts: the diminution of both the vernier and stereoscopic thresholds with increasing length of the lines in the test object, the improvement of vernier acuity with eye movement, and the superiority of vernier acuity in binocular as compared with monocular vision. In all these cases, the decrease in the threshold goes parallel with an increase in the number of elements to be averaged, and thus with a higher precision of the 'mean local sign.'

Essentially the same conception of an interaction process between 'space values' or local signs has been developed independently by Hofmann (32), who brings forward a very interesting line of experimental evidence in support of it.

We may remark that the theory of mean local sign can be applied to any situation in which we test the delicacy of visual response to a difference of position between two independent patterns. In the case of vernier acuity, this independence is guaranteed by the end-to-end placement of the lines; in the case of stereoscopic perception, by the fact that the contours to be discriminated are presented to different eyes. We may also suggest that the theory would equally well apply to the perception of movement, and to the perception of small differences in the breadth of lines, and would account for the fineness of the thresholds obtained in these cases.

If this conception is correct, there is a very important difference, from the point of view of theory, between the ordinary visual acuity (*Auflösungsvermögen*) and the function which Hering designated as the *Raumsinn*. While the former is primarily dependent on brightness perception, the latter depends, in a much more direct way, on the organization of the visual mechanism with respect to *spatial* discrimination[1].

VI. VISUAL ACUITY AND ILLUMINATION.

The effect of illumination on the resolution threshold has been studied by many experimenters, beginning with Tobias Mayer in 1754. The best known of these studies is that of König (37). König employed a black Snellen E-figure upon a white background as his test object. The figure and its ground were visible by reflected light, the same external illumination falling upon both. This illumination was varied through a wide range, and the effect of this variation upon the acuity was observed.

Throughout the experiments the proportions of the E-figure were kept constant. In order to test the acuity, the visual angle subtended by the figure as a whole was decreased (either by increasing the distance of the observer from the test object, or by diminishing the absolute size of the object itself) until it could no longer be resolved. (The orientation of the E-figure was changed from observation to observation, and each time the size of the retinal image was progressively decreased until the observer could not correctly identify the orientation.)

König represented his results in the form of a graph in which acuity was plotted against the logarithm of illumination. This graph consisted of three straight lines: a line of small slope, representing the data for the lowest illuminations, a second line of steeper slope, for the medium illuminations, and, for the highest illuminations, a third line which was nearly horizontal. He considered the first line to represent the acuity furnished by the retinal rods, the transition to the second line occurring at the intensity at which the cones come into function. While accepting König's interpretation of his results in terms of the duplicity theory, Hecht (24) considers that the data can be most adequately fitted by a single smooth curve of the ogive form.

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[1] This conclusion is also suggested by the finding of French (18) that the vernier threshold (unlike the resolution threshold) is but slightly affected by changes in illumination.

The following observation by Gelb and Goldstein (20), on a case of brain pathology, is interesting in this connection. "In a subject with 'psychical blindness' the ordinary visual acuity (resolution threshold) was found to be approximately normal. When, however, the subject was required to discriminate between a straight line and a slightly bent line, his discrimination was decidedly inferior to the normal person's."

Other experimenters[1], employing different test objects and otherwise different conditions, have obtained a variety of empirical curves and formulae representing the relation between acuity and illumination. In general, their procedure has been broadly similar to König's.

A few remarks should be made about the implications of this procedure. The resolution threshold, as we have previously noted, depends upon the brightness of the test object and upon the brightness of its background. According to König's method, both of these brightnesses are varied simultaneously, and the change of acuity represents the composite effect of both factors. Moreover, and this is perhaps still more important, König's method involves a variation in the size of the test object as a whole, and not simply of the interspace which is to be resolved.

Thus König's data, and other data obtained by a similar method, do not represent the isolated effect of one particular variable, namely, the intensity of the white background, but involve other important variables in addition. This complication is unfortunate in so far as the theoretical interpretation of the results is concerned[2].

Under the customary experimental conditions, one generally finds that acuity increases with illumination (though eventually reaching a level beyond which there is no further change, as in König's experiments). Various theories have been proposed to account for this change of acuity. Many writers have referred it to the dioptric errors of the eye, these errors being of less consequence at high than at low illuminations, for certain physiological reasons. Thus Hofmann ((31), p. 41) says: "The dependence of visual acuity upon light intensity has especial theoretical interest, inasmuch as it furnishes a clear proof of the lack of sharpness of the retinal images, even in the normal, well-accommodated eye."

According to Hering, the distinctness of vision rests upon simultaneous contrast, which compensates for the defects of the image-forming mechanism. He attributes the greater distinctness at high intensities to an increased degree of contrast ((30), p. 157). It can hardly be said, however, that either Hering or Hofmann has given a clear and adequate treatment of the concrete problem with which we are concerned, namely, the effect of light intensity upon the resolution threshold. Hering has, however, suggested certain ideas which offer themselves as a basis for such a treatment (28). In a later paper we shall examine these ideas and consider their possibilities for the development of a satisfactory theory.

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[1] A number of references to the literature are given by Guillery ((21), pp. 778 f.).

[2] It has been suggested by Chalmers (10), Roelofs and Bierens de Haan (49), and Kühl (41), that areal summation of stimuli plays an important part in determining the form of the acuity-intensity curve. We may cite the fact demonstrated by Abney (1) that the apparent brightness of a small, weakly illuminated area is a function of its size.—In any case, the influence of absolute size on acuity, even at high intensities, is well known.

Hartridge (22) has suggested that the change of acuity with illumination is due to the failure of the eye to obey Weber's law for brightness discrimination. But this hypothesis, which demands that acuity should always be low at very high as well as very low intensities, is not in accord with experimental facts.

VII. HECHT'S THEORY OF THE RELATION BETWEEN ACUITY AND ILLUMINATION.

We now turn to the theory of Hecht, which represents an entirely novel attack on the problem.

It is generally admitted that visual acuity is limited by the mosaic structure of the retina, in the sense that this structure establishes a certain maximal value which acuity cannot surpass. But acuity varies with illumination, and falls far short of the ideal or maximum value when the brightness conditions are unfavourable. This has been taken as an indication that retinal structure, however important it may be, is not the sole factor regulating acuity.

Hecht, on the other hand, suggests that the change of acuity with illumination may be due to an actual variation in the distribution of receptors. Of course the number of rods and cones in the retina is anatomically fixed. But one may suppose that this number varies in a *functional* sense.

The retinal elements, it is assumed, vary in their sensitivity to light. Only the most sensitive receptors will function at the lowest intensities. As the intensity is increased, more and more receptors will respond. It is for this reason that visual acuity increases with intensity; the number of functional elements per unit area is increased, or the retinal 'grain' becomes finer. Visual acuity is assumed to be proportional to the number of functional elements in unit retinal area.

The data of König indicate that the relation between acuity and *log* illumination is ogival. Hecht proceeds to show how König's data can be quantitatively explained by his theory.

He assumes, in conformity with his earlier work (23), that there exists in the rods and cones a photochemical substance which is decomposed by light. This decomposition is a bimolecular reaction, its rate being proportional to the intensity of illumination. Moreover, the reaction is reversible. On these assumptions it can be deduced that under prolonged illumination by the intensity I, the relation between concentration of decomposition product (x), and I, is given by the equation

$$KI = \frac{x^2}{a - x},$$

where a and K are certain constants.

It is supposed that the accumulation of a given concentration of decomposition products in a receptor cell discharges the cell and sends an impulse along the corresponding optic nerve fibre. But this threshold concentration will vary from one cell to another. Hecht postulates that the thresholds of the various cells are uniformly distributed with respect to concentration x. In other words, given a concentration x in the entire population of cells, the total number which will be active at this concentration is a linear function of x. But x, according to the photochemical theory, is related to the intensity I by the equation given above. It will be seen, therefore, that the theory provides a basis for calculating the number of functional elements at any value of illumination.

The situation is complicated by the fact that the retina contains two sorts of elements, rods and cones. It is assumed that the dynamics of the photochemical system are similar for the rods and cones, although the photosensitive substances are not necessarily the same. To quote Hecht:

> Beginning with the lowest illuminations, vision is a function of the rods. The number of rods which are active is very small; this amounts to having a resolving surface with the receiving elements sparsely distributed. The retinal distance between two just discriminable contours must be large, and visual acuity is very low. As the illumination increases, more and more rods reach their threshold and become functional. The average distance between the active elements becomes smaller, and visual acuity larger.
>
> Presently an illumination is reached when the first cones begin to function. Visual acuity will still be mediated by the rods because there are already more functional rods present than cones. But... the rate at which the cones come into play with increased illumination is nearly ten times as great as the rate of the rods. Therefore at a certain point the number of cones functioning in the fovea will be equal to the number of rods in the periphery. As the intensity increases beyond this, the number of active foveal cones per unit area will be greater than the number of active peripheral rods in an equal area, and visual acuity will now be determined by the cones. This augmentation of the number of functional cones and the concomitant increase in visual acuity will continue until all the cones are active, and no further change in visual acuity is possible ((24), pp. 261 ff.).

Hecht shows that, on the basis of these assumptions, a theoretical curve representing visual acuity as a function of *log* illumination can be drawn, and this curve fits the data of König remarkably well. He also shows that a curve of the same form (although with different absolute values of visual acuity) fits the more recent data of Roelofs and Zeeman (50).

According to the theory, the visual acuity of a totally colour-blind (cone-blind) person should be calculable from the distribution of thresholds for the rods alone. And the actual data of König and Uhthoff on the acuity of the totally colour blind are found to confirm this expectation.

The experiments of Hecht and Wolf (26) on the visual acuity of the honey bee show that the relation between visual acuity and illumination is rather similar to that found by König for the human eye; when acuity is plotted against *log* illumination, a sigmoid curve is obtained. This result is particularly interesting, inasmuch as the insect eye is built upon a quite different plan from that of the human eye, and possesses no dioptric mechanism analogous to the crystalline lens. Moreover, Hecht and Wolf find that the lowest resolution threshold, obtained under high illumination, corresponds almost exactly to the diameter of the narrow central elements in the mosaic eye. If these fine elements are painted out, the ordinates of the visual acuity curve are all reduced. "If visual acuity is always mediated by the region of minimum angular separation, then the elimination of this central foveal area should depress the visual acuity function to the level of the remaining peripheral ommatidia" ((26), p. 182). According to anatomical data, the minimal separation of the functioning elements in this case is about 3/4 that existing in the intact eye. And the visual acuity curve in fact has all its ordinates reduced to almost precisely 3/4 of their original values. Thus, experiments performed upon a different type of eye appear to demand an explanation similar to that which was worked out for the human eye.

If, with Hecht, we suppose that the number of functional receptors changes with intensity, we may also expect that *intensity perception* will be influenced by this number. Although recognizing that the frequency of optic nerve impulses will also change with intensity, and will play a certain rôle in intensity perception, Hecht believes that the *discrimination* of differences in intensity is based upon *the number of functional receptors*. In other words, if we start with a dark field, and gradually add more and more light to it, each distinguishable increase in brilliance represents the addition of one new functional element per unit area.

Hecht proceeds to work out the quantitative consequences of this idea. He suggests, to begin with, that the term 'unit area' may be given more than a purely formal meaning. There may be such a thing as a real unit area in a physiological sense. He says ((24), pp. 275 f.):

> Is there a minimal retinal area which contains the equipment for recording the various properties such as intensity perception, colour vision, visual acuity, and the like, usually ascribed to the eye as a whole? One can, in terms of the ideas presented in this paper, make a calculation of the size of such a minimal retinal area.
>
> As a direct result of his experiments with Brodhun on intensity discrimination König computed that it is possible to recognize 572 discrete steps in brightness over the complete range of intensities perceptible to the eye[1]. By comparing his computations with an analysis of intensity discrimination (Hecht, 1924–25), it is apparent that the first thirty of these steps are mediated by the rods; the remainder, 542, by the cones. If the recognition of a minimal intensity difference represents a change from n to $n + 1$ or $n - 1$ in the number of cones functional in a unit area, then the *minimal* retinal area in the fovea must contain 542 cones. The actual unit area may be some multiple of this; but the *minimal* area can contain no fewer than 542 cones. This number is thus derived directly from the experimental data of intensity discrimination.
>
> The minimal retinal area should also be able to mediate all visual acuities, from the highest to the lowest. Such an area will have for its side the retinal distance between two just perceptible contours corresponding to the lowest visual acuity. The retinal distances for all other values of the visual acuity will obviously fall within this distance. Moreover this minimal retinal area for visual acuity is by definition the same for rods as for cones, since the unit of visual acuity is taken to be the same for both. From König's data..., the lowest visual acuity is 0·03 units. König's unit of visual acuity corresponds to a visual angle of 1′ 20″. Therefore a visual acuity of 0·03 refers to an angular separation of slightly over 44′, and to a retinal distance of 0·2 mm. The resulting square area[2] is 0·04 sq. mm. The fovea contains 13,500 cones per sq. mm. (Helmholtz, 1866). The minimal area derived directly from visual acuity data therefore contains 540 cones, which is the same number as that derived from intensity discrimination.
>
> From [the curves representing the distribution of thresholds], it is apparent that in the normal eye the cones take over the mediation of visual acuity from the rods when only half of the rods are functional. There must therefore be about 60 rods per unit area in the retinal periphery in order to account for intensity discrimination. This is a ratio of 1 : 9 of rods to cones. Note the same relationship in visual acuity. The maximum cone visual acuity is 1·71, the maximum rod visual acuity is 0·21. This is a ratio of 1 : 8 + of rods to cones....

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[1] According to König's own statement, however, the figure 572 represents the number of steps comprised in the range of intensities actually covered in the experiments, and not the number of steps in the complete range perceptible to the eye. By a process of extrapolation, König deduces the value 660 for this latter number.

[2] It is not entirely obvious why one should take "the resulting square area," rather than, *e.g.* the resulting circular area. In point of fact, the *shape* of the unit area would seem to be indeterminate.

It should be pointed out that the ratio of rods to cones in a unit retinal area corresponds with the findings in the photochemical system suggested as a basis for the sensibility distribution of the retinal elements. For a minimal intensity discrimination it was found (Hecht, 1924–25) that in the cones this means an increase of 0·20 units of photochemical decomposition, whereas in the rods the increase is 1·6 units. Since there are 100 units in each photochemical system, there are possible 500 photochemical steps for the cones and 60 for the rods; again a ratio of 1 : 8 + of rods to cones.

Thus, on the assumption of a physiological 'unit area' which is capable of mediating all visual acuities, and all visual intensities, from the lowest to the highest, and on the assumption that each just noticeable difference in visual intensity represents the addition of a single functional element per unit area, one arrives at a quantitative agreement between the data of acuity and the data of brightness discrimination.

Houstoun (33) has recently proposed a modification of Hecht's theory. While accepting Hecht's basic idea that acuity, as well as intensity discrimination, depends on the number of functional elements, he thinks it erroneous to correlate acuity with the number of elements per unit *area*. Acuity, which is inversely proportional to the linear separation of the just discriminable contours, should also depend on the linear separation of the functional retinal elements, in other words, upon the number of functional elements per unit *length* of the retina. Intensity perception, on the other hand, is based on the number of active receptors per unit area. Visual acuity is proportional to the square root of the latter number.

Houstoun and Shearer (34) have obtained new experimental data on both visual acuity and brightness discrimination, and they find that these data agree much more closely with their own theoretical assumptions than with the assumptions of Hecht.

These writers further point out that in the usual acuity tests, both the test object and its background are illuminated; acuity depends on the *difference* in intensity between object and ground. In the theory of acuity, account must be taken of the magnitude of this difference, and not merely of the strength of illumination incident on the test field.

Houstoun's conception of the nature of the receptor process is quite different from Hecht's; but for our purpose it suffices to note that he adopts, with modifications, Hecht's idea that the number of active retinal elements varies with intensity, and that this variation forms the basis of brightness discrimination and of visual acuity.

We have already pointed out, in discussing the relation of acuity to the retinal mosaic, a number of difficulties in the way of any theory which, like Hecht's and Houstoun's, makes acuity depend solely on the distribution of responding elements. Similar difficulties have been mentioned by other writers. Thus, Best (8) has remarked that Hecht's theory omits to take account of the gross imperfections of the image-forming apparatus of the eye, besides neglecting the important factor of interaction of visual areas (simultaneous contrast). Koffka and Harrower (36) have called attention to the dependence of acuity on the size and form of the test object[1].

We believe that the most fundamental objection to the theory lies in the fact that it fails to consider the actual perceptual phenomena which are involved in the recognition of detail. Consider, for example, a test object consisting of a series of alternate light and dark lines. If the lines are very fine, we perceive simply a patch of homogeneous grey. As the width of the lines is increased, we eventually see a non-uniform field—a series of lines of dissimilar brightness. The 'resolution' of the test object represents the perception of this inhomogeneity of brightness within the field.

A theory of acuity must start with the question: What are the conditions under which this gradient of brightness becomes perceptible? Now it is clear that if the lines are too fine in relation to the size of the retinal elements no brightness differences can be seen. But, on the other hand, if we take considerably coarser lines, we can make them imperceptible by diminishing the objective brightness gradient between the two sets of lines (13). In general, we can conclude that any factor affects acuity only in so far as it affects the perception of this gradient[2].

Thus the analogy which Hecht makes between visual acuity and the resolving power of a photographic plate is an imperfect one. Acuity is not wholly, or even primarily, dependent on the retinal 'grain,' and we cannot argue that every change in acuity presupposes a corresponding change in the distribution of sensitive elements.

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[1] Freeman (16) objects to the theory on the ground that acuity depends on central and not merely on retinal factors. The acuity of the same retinal region may vary with the *distance* separating the observer from the stimulus field, even though the retinal image, and its illumination, are constant. Other facts might be cited to show that acuity is dependent on central conditions. It has been shown by Gelb (19) that the acuity of a given peripheral spot is affected by the form of the total stimulus pattern. Kravkov has found that the visual acuity of one eye is influenced by simultaneous stimulation of the other (39), and that acuity is altered by the presence of a simultaneous acoustic stimulus (40). The superiority of binocular over monocular acuity (15) is probably not wholly due to ocular factors.

[2] A more complete theory of acuity should include the case where colour differences, as well as brightness differences, are involved. If, *e.g.*, our test object consists of alternate grey and coloured lines of equal brightness, its resolution corresponds to the perception of a chromatic rather than a brightness gradient. For a criticism of Hecht's theory, from the point of view of chromatic acuity, cf. Koffka and Harrower (36).

In particular, then, we cannot take it as a self-evident fact that strength of illumination affects acuity only in so far as it causes a change in the number of functional receptors. For we have to reckon with the alternative possibility that the effect may be due to a change in the perceptibility of the light gradient on the retina.

Hecht justifies his conception of the physiological basis of the acuity-illumination relation on the ground that it leads to a numerical prediction of the experimental facts—namely, the data of König. This quantitative evidence would be more convincing if it were actually true that a one-to-one relation obtained between acuity and illumination. But since, even for one and the same value of illumination, the acuity may vary within wide limits—depending on the difference in reflection factor between test object and background, the size and form of the test object, and so on—our attitude must remain sceptical.

We agree with Hecht, however, that no satisfactory and well-worked-out alternative theory has been proposed. We also share his belief that an adequate theory should supply a quantitative and not merely a qualitative description of the facts. Accordingly, we have thought it worth while to undertake a new investigation of the problem, in the hope of arriving at such a theory. The results of this study justify the conclusion that the factor invoked by Hecht (as well as by Houstoun), namely, a change in the number of functional receptors, is not required in order to explain the dependence of acuity upon intensity.

But, for the sake of completeness, we ought to consider Hecht's theory from another point of view. Even though there is no evidence for the belief that the number of responding elements changes with intensity, still this does not preclude the possibility that this belief may be correct. Now, according to Hecht, the number of functional elements per unit area furnishes the basis for *brightness discrimination.*

We have said that the perceptual phenomenon involved in visual acuity tests is the recognition of a difference in brightness between neighbouring small areas. But what is the physiological basis of brightness perception? We might say, with Hecht, that the neighbouring areas are discriminated because they contain different *numbers* of functional elements, *e.g.* the perception of a dark line on a bright ground might conceivably be due to the fact that the line stimulates fewer elements per unit area than does the ground.

Clearly, this modified form of the theory which we have suggested is not open to the same objections which confront Hecht's attempt to correlate acuity with distribution of receptors, and it may well be compatible with a variety of ideas regarding the basis of the acuity-intensity relation.

The alternative, and more orthodox, view would be that the number of functional elements is subject to no such gross variations, and that brightness perception depends primarily on the way in which individual elements respond to altered stimulation. We must now consider the relative merits of these two different conceptions regarding the basis of brightness perception[1].

It has been shown by Adrian and Matthews (2) that the frequency of impulses in the optic nerve increases with intensity of stimulation. It would appear, then, that brightness perception finds an adequate basis in impulse frequency, and that there is no immediate necessity for postulating a variation in the number of functional receptors. Hecht himself recognizes that frequency is a factor in intensity perception, but assigns it a rôle of minor importance ((25), pp. 261 f.).

According to Hecht, "it is difficult to consider frequency alone as the basis of intensity discrimination. Frequency is a continuous function because time is continuous. But it is a fundamental property of intensity recognition that it is discontinuous..." ((25), p. 253). We believe, however, that the differential threshold for brightness can scarcely be considered as a true 'quantum.' Given two slightly different intensities, the probability of correctly judging the direction of their difference is experimentally found to be a continuous function of the magnitude of the actual difference (17). It is difficult, therefore, to accept the existence of a threshold as evidence for the number-of-elements hypothesis. In any case, the brightness threshold finds its analogy in a vast variety of sensory and perceptual thresholds, and this fact in itself may well make us doubt the necessity of any such *ad hoc* hypothesis.

It is altogether probable that there is at least some slight variation in the thresholds of the retinal elements, so that they do not all come into function at precisely the same intensity. Doubtless, there are differences between the thresholds of different elements at the same instant, and of the same element at different instants. There is, however, no evidence in favour of the very gross variations which are postulated by Hecht; according to his theory, *e.g.* there are certain elements which are stimulable only by the highest visible intensities.

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[1] There is, of course, the possibility that both the number of active elements and the response of each active element may vary with intensity, and that the perception depends on the joint effect of both these factors.

Now, as we have pointed out elsewhere (58), this theory leads to a contradiction when we consider it in relation to the eye's power of perceiving minute stimuli. A single dark spot on a white background is visible (in moderate daylight illumination) when the breadth of its schematic image on the retina is smaller than the diameter of a single cone.

The perception of such a spot must rest on the fact that a single cone receives less light than the cones surrounding it. In other words, one cone is either non-functional, or functions in reduced degree, and the subtraction of this single element produces a perceptible discontinuity in the otherwise continuous surface of the visible background. But this, in turn, implies that the light reflected by the white paper is sufficient to evoke a response from the entire mass of receptors which it strikes. For if (as Hecht's theory implies) only a fraction of these receptors were called into function by this illumination, then each *non-functional* cone would give rise to the perception of an apparent discontinuity in the surface of the background. That is to say, the background would be seen not as a continuous surface but as a mosaic of light and dark spots. Since we actually receive no such impression from a surface of white paper illuminated by daylight, we are led to the conclusion that all the foveal cones, and not merely a fraction of them, respond to this stimulus.

But the perceptual intensity, or brilliance, corresponding to this stimulus is very low in relation to the total range of possible brilliances. We infer, therefore, that throughout at least a very wide range of stimulus intensities all the cones are functional, and hence that the increases of brilliance with stimulus intensity which occur within this range are mediated not by the addition of new functional elements but by variations in response on the part of elements already functional.

This conclusion, that number of elements is not the sole basis of brightness discrimination, is confirmed by the following experiment (58). A black and a grey spot were simultaneously presented on a white background, the schematic retinal image of each spot corresponding to the size of a single cone. Despite the smallness of the images, it was found possible to discriminate the brightnesses of the two spots.

It still remains possible that a variation in receptor thresholds may be an important factor in light discrimination at weak intensities. Further investigation must decide whether or not this is the case.

The present critique concerns itself with Hecht's theories only in so far as they deal with problems of human vision. It is obvious that the problem which is raised by Hecht and Wolf's interesting experiments on insects requires separate consideration. The validity of Hecht's explanation as applied to this problem is a question which we must leave open.

VIII. SUMMARY.

1. The resolution threshold is defined as the visual angle separating two neighbouring contours when they can just be recognized as discrete. Visual acuity, by definition, is inversely proportional to the resolution threshold.

2. It is pointed out that the resolution of a test object is based on the perception of a difference in brightness between the contours and their interspace. Thus there can be no sharp distinction, from the point of view of theory, between the *minimum separabile* and the *minimum visibile*.

3. The vernier threshold and the stereoscopic threshold, on the other hand, probably do not depend so directly upon brightness perception, but are determined by the delicacy of the mechanism underlying localization.

4. The structure of the retina places certain limits to the perception of brightness gradients, and hence to visual acuity. It is uncertain whether these limits are ever attainable in practice. Under the usual experimental conditions, the resolution threshold falls considerably short of the anatomical limits. The importance of optical blurring and of simultaneous contrast for visual acuity is emphasized.

5. The dependence of acuity on light intensity is discussed, with particular attention to the recent theory of Hecht. It is pointed out that Hecht's analogy between visual acuity and the resolving power of a photographic plate is an imperfect one; the theory fails to take into account the concrete perceptual situation involved in the recognition of detail. Its assumptions are inconsistent with the fact that acuity may vary within wide limits even though illumination remains constant.

6. Hecht's postulate of a gross variability in the thresholds of the retinal elements is contradicted by the results of experiments on the perception of minute areas.

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RHYTHMIZATION AND SPEED OF WORK[1].

By D. W. HARDING.

(From the Research Laboratories of the National Institute of Industrial Psychology.)

I. *Introduction* (pp. 262–3).
II. *Use of the term 'rhythm'* (pp. 263–4).
III. *Divergent uses of the term* (pp. 264–5).
IV. *Rhythm in typewriting* (pp. 265–6).
V. *Method of measuring rhythmization* (pp. 267–8).
VI. *Treatment of results* (pp. 268–70).
VII. *Rhythm and speed* (pp. 270–2).
VIII. *Effect of training in uniformity* (pp. 272–3).
IX. *Erratic workers* (pp. 273–4).
X. *Experiment in indicating rhythms to learners* (pp. 274–6).
XI. *Results of the experiment* (pp. 276–7).
XII. *Summary* (p. 278).
References (p. 278).

I. INTRODUCTION.

THE value of rhythm in work seems never to have been in doubt. Unfortunately, however, general agreement on this subject is largely invalidated by differences in the meaning assigned to the word 'rhythm' by different writers. Moreover, the extent of the aid given by rhythm is extremely difficult to assess. This difficulty arises chiefly from the lack of any exact means of measuring the degree of rhythm present in a given performance of a task.

In the work to be described an attempt was made to measure certain aspects of rhythm in the performance of a muscular task and to relate them to the efficiency of the performance. The task observed was typewriting, but it seems likely that the importance of rhythm here is paralleled in many other operations, particularly in those that consist in the reproduction of language: linotype composing, telegraphy, and perhaps the writing of longhand and shorthand. In the acquisition of speech

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skills also rhythm is probably of the utmost importance. In these tasks a large number of different movement sequences, in the form of words and phrases, recur at frequent intervals but are not repeated continuously. The patterns of the movement sequences are therefore not thrust on the worker in the same way as the patterns of simpler repetitive work. In some simple factory tasks it seems almost impossible not to rhythmize. In typewriting it is much less nearly inevitable. In typewriting, consequently, individual differences in rhythmizing are far more marked and there is a greater possibility of relating them to differences in efficiency.

II. USE OF THE TERM 'RHYTHM.'

The study of rhythm in typewriting brings up urgently the question of what 'rhythm' means. Recent writers have diverged widely and in various directions from the usage established by earlier workers, and considerable confusion has resulted. An attempt has been made in the present paper to return to the conception of rhythm found in the papers of such workers as R. MacDougall (5), and Miner (6), who themselves quote earlier authorities.

These writers agree with each other reasonably well in what they regard as the essential features of rhythm. In the first place it is agreed that rhythm is a unique kind of unifying activity: a number of impressions that would otherwise be merely a sequence can, if rhythmized, be attended to as a unit. This unification of impressions is independent of their having any other perceptual significance; a sound sequence can be quite meaningless but still form a rhythmical unit. Further, the unification brought about by rhythm is, for consciousness, immediate, and though conscious activities (such as counting) may help to bring it about, yet when it does occur it will appear as an "immediate fact of sensory apprehension" (R. MacDougall (5)). There is also very general agreement that the rhythmized impressions are differentiated according to their position within the rhythm unit; some 'stand out' and others are subordinate. Something comparable to biological specialization is involved. Thus, for instance, a brief burst of machine-gun fire, though perceived as a unit, fails to form a rhythm because its component sounds are not differentiated; instead of being integrated within the unit, they are merely enclosed within it.

Expressing these points of agreement briefly, we may say that rhythmization shows itself, if it becomes available to introspection, as an immediate grouping of a series of impressions, together with a differentiation of the component members of the group. A further important fact, noted by many workers, is that once rhythmization in a certain form has been established it tends to recur in that form extremely readily.

III. DIVERGENT USES OF THE TERM.

The above definition of the term excludes certain meanings which the word 'rhythm' has been given in current psychological writings. Among them may be noted the meaning 'smoothness of movement' which is implied in parts of Farmer's work (1, 2). General psychological usage seems never to have given the term this significance. Münsterberg, indeed, in *Psychology and Industrial Efficiency*, speaks of rhythm being *lost* through the introduction of machines when "the rhythmical horizontal and vertical movements were for physical reasons usually replaced by uniform circular movements." The complete smoothing of movement eliminates rhythm.

Sargant Florence in *The Economics of Fatigue and Unrest* defines rhythm as "the regular and frequent repetition of a group of differentiated motions and pauses." This, too, seems at variance with most psychological writers. A group of differentiated motions and pauses may certainly be regularly and frequently repeated, but it is then the rhythm which is repeated, not the repetition which constitutes the rhythm. Throughout his discussion of rhythm Sargant Florence uses the term as a synonym for 'regularity of repetition.' This is one of the commonest usages, but it seems to have no psychological support. It is frequently found in accounts of the effect on output and fatigue of introducing conveyers, or otherwise prescribing the rate of work. Here the *fixed timing* is spoken of as rhythm.

Speed or tempo of movement is a third meaning often assigned to 'rhythm.' Münsterberg, for example, says: "Motions in the rhythm of 40 to the minute were on the whole just as exact as those in the rhythm of 20." Quite evidently the word 'tempo' would fit his sense more accurately. It is this use of the word that is usually, though not always, implied in references to 'the natural rhythm of the worker.'

A fourth use is made of the word by Wyatt and Weston (8) and by Vernon (7). These writers define it as the ratio of muscular contraction to relaxation, or of work to rest. This, too, lies outside the meaning of 'rhythm' as the word has been used by psychologists. Different rhythms might of course be set up by tasks that involve different ratios of contraction to relaxation, but the ratio would be only a mathematical statement of the form the rhythm took. Rhythmization, whether the rhythm is imposed on a uniform series of impressions or suggested by differences in the series, is an activity carried out by the organism, and not a mathematical conception.

IV. RHYTHM IN TYPEWRITING.

In popular speech, rhythm in typewriting has come to mean exclusively the maintenance of uniform time intervals between successive strokes, and of uniform force in striking the keys. This usage is extremely misleading, since it is exactly the reverse of that employed in the earlier psychological work on rhythm. In that work nonsense syllables, for instance, were said to be rhythmized when the uniform series in which they were presented was broken up into groups, or patterned. And this early usage is clearly in line with the accepted psychological conception of rhythm. In the work to be described, therefore, rhythmization is supposed to occur not when the key strokes form a perfectly uniform series, but when they are differentiated and grouped. It was found that most typists do quite markedly rhythmize their movements. Phrases and long words are divided into groups of strokes, quick runs of letters being followed by slight pauses. Different combinations of letters produce distinctive patterns (there is no constant grouping into threes or fours, for instance), and the same pattern is—allowing for individual differences in degree of consistency—adhered to whenever the word or phrase occurs. Marked differences exist among individuals, both in the extent to which they stress the pattern, and in the consistency with which they adhere to it in later repetitions of the word. Some individual differences in the form of the pattern adopted for a given word are found, but the main features of the pattern are fairly constant from one subject to another.

These main features of the pattern are suggested by the comparative convenience on the keyboard of different combinations of letters, and not by ease of spelling or pronunciation. The word 'enclosed,' for instance, is generally split up thus: 'encl-os-e-d.' The main principle underlying the way in which a word or phrase is divided is that demonstrated by Lahy (4), namely, that the interval between letters struck with alternate hands is shorter than the interval between letters struck with different fingers of the same hand.

Is one justified in calling this grouping 'rhythmization,' when it is suggested by the position of the keys? The enormous differences among the subjects in the extent to which the pattern is emphasized, and in the consistency with which it is adhered to in successive repetitions, leave no doubt that some personal factor, apart from the arrangement of the keyboard, enters into the results. But it may be suggested that this personal factor is simply one of neuro-muscular peculiarity which either aggravates or diminishes the effect of the keyboard arrangement. This possibility has not been experimentally disproved, but it seems unlikely, and in any case it is of no great importance for the present paper. The important point is that, however the grouping may have originated, once it has originated it ceases to be a mere mechanical grouping imposed on the subject, and becomes instead the form in which the subject expresses her active effort of rhythmization or integration. Some degree of rhythmization is assumed to be present when a particular grouping of strokes is adhered to, even in the absence of confirmation from the subject's introspections. (The same assumption is implied in the definition by Sargant Florence quoted above.) Some of the subjects who grouped their letter strokes consistently, reported, on being questioned, that they did feel the pattern or rhythm; others said that they did not. Even these latter subjects are assumed to have rhythmized. That is, it is assumed that some activity was going on at non-conscious levels, and would, if it had been more prominent or better attended to, have been experienced by the subjects as rhythmization.

Two reasons may be advanced for supposing that this is so. In the first place, it is difficult to see why the degree to which the grouping is stressed, if it were merely imposed by the keyboard arrangement plus the subject's neuro-muscular peculiarities, should be positively related to her capacity for speed (see VII below). It might equally well be expected to retard speed. The fact that it is positively related to speed, strongly suggests that the grouping is *rhythmical* (*i.e.* that the subject does not merely acquiesce in it, but actively exploits it as the most convenient form into which to cast her rhythmizing); the greater speed can then be explained as being due to the extra integration given to the movement-sequence by the rhythm. Secondly, there is some evidence (see XI below) that speed can be improved if the typist wittingly adopts a suitable rhythm. If, therefore, one knows that a grouping which certainly is rhythmical tends to improve speed, there is considerable likelihood that a similar grouping which aids speed is also rhythmical.

For these reasons it has been supposed that the patterning observed indicates that the movements have been rhythmized.

V. METHOD OF MEASURING RHYTHMIZATION.

In the present investigation, observations were made of the time intervals that elapsed between successive letter-strokes when typists practised one word repeatedly. An ordinary portable typewriter was used, but in place of a sheet of paper for receiving the impressions there was substituted a paper tape moving through the machine at a constant speed. The subjects typed in the usual way. Since the tape moved continuously (regardless of the spacing mechanism), any difference in the time intervals between strokes was recorded as a difference in the spacing of the impressions on the tape. No direct account was taken of differences in the force exerted in the strokes. It was assumed that these differences would produce corresponding differences in time interval. This is confirmed by the findings of Lahy (4) on the "Relation between period of contacts, period of intervals, and force of stroke."

Four groups of subjects, comprising seventy-nine in all, were observed. All were women. Group I consisted of twenty professional typists who differed among themselves in training, methods of fingering, and length of experience, but who had all reached ordinary business standards and were employed in a large office. The other three groups consisted of students from a commercial training school. Group II comprised six students who had been trained for a year and a half and had practically reached business standards; Group III comprised twenty-eight students with one year's training; Group IV comprised twenty-five students with one term's training. All the students had been trained in 'touch' typewriting and were familiar with the keyboard. They had also been encouraged throughout their training period to adhere to uniform time intervals between successive strokes; most had done a little 'typing to the gramophone' (*i.e.* keeping in time with records that inculcate absolute uniformity of time interval), but the importance of uniformity had not been stressed as strongly as it sometimes is in modern teaching.

Each subject was observed individually. She first typed for some time a piece of ordinary prose, in order to familiarize herself with the experimental situation. She then proceeded to the intensive practice of two words, 'hormone' and 'heterogeneous,' taking them one at a time and typing them again and again, 'hormone' about forty times and 'heterogeneous' about sixty times. A specimen of the word was always in front of her for reference. The subjects were told to aim at reaching their normal speed for rapid work. A rest of a few minutes was made during the practice of each word, and the subjects were encouraged to make comments on their progress and methods.

VI. TREATMENT OF RESULTS.

Fig. 1 shows successive repetitions of a word by one subject: these have been cut from the tape, and placed in order one below the other. For convenience of inspection the position of each letter is then marked by a point, and the repetitions shown in the form of a chart (Fig. 2). A line connects each letter in one repetition with the same letter in those following. This provides a convenient graphical demonstration of the time relations among the letter strokes. If the same interval between two letters has been maintained for two or more repetitions, the lines connecting these letters will be parallel.

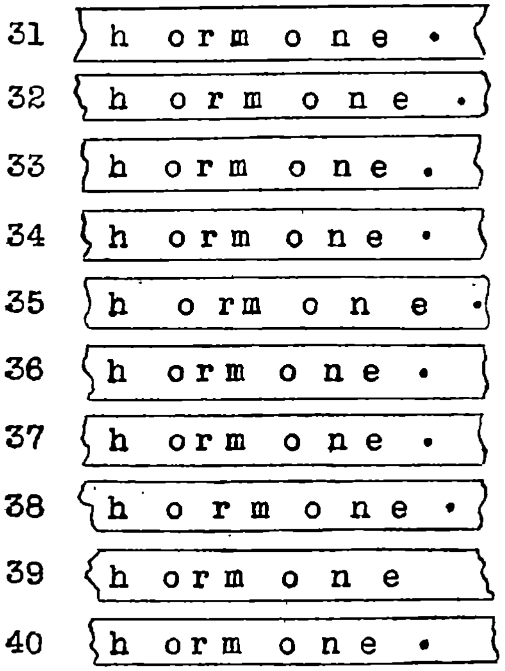
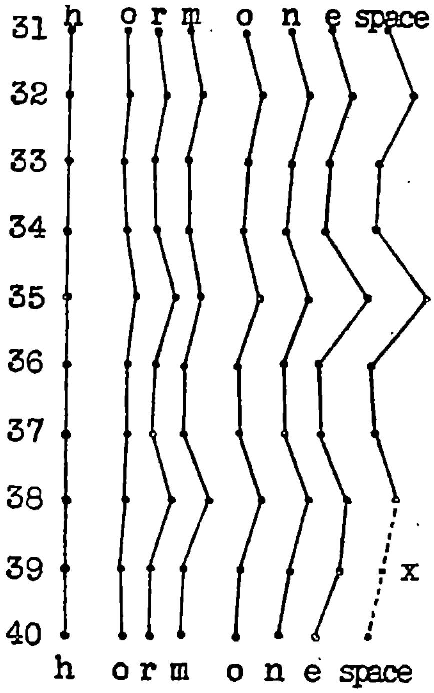

31 h o r m o n e
32 h o r m o n e
33 h o r m o n e
34 h o r m o n e
35 h o r m o n e
36 h o r m o n e
37 h o r m o n e
38 h o r m o n e
39 h o r m o n e
40 h o r m o n e

Fig. 1. Ten successive repetitions of a word by one subject.

For the comparison of one subject with another some mathematical statement of the results is useful. It is first necessary to measure the subject's *deviation from uniformity* of time interval between one stroke and the next. If the subject has rhythmized her movements, this measure will indicate how much the rhythm has been emphasized. If she has been merely erratic, the same measure will show how erratic she has been. To decide whether a subject who deviates from uniformity is erratic or rhythmic, one must have a measure of the *consistency* with which she adheres, in successive repetitions of the word, to the same time relations among the letter strokes.



× Space bar omitted

Fig. 2.

As a basis for these measures the ratio of each interval to the next was taken, in preference to the absolute interval. This was done on the ground that variations in the speed of typing the word as a whole would increase the variability of the absolute intervals, although conceivably the same time relations, or pattern, might be maintained at the different tempos. All measures were made on the ten fastest correct repetitions of the word that the subject achieved. This excluded repetitions where one interval was abnormally long owing to some chance difficulty. As the subjects were generally satisfied that they could get no faster with further practice, the measures thus obtained may be taken as representative of the subject's performance at top speed. Apart from the fact that the measures were made only on correct repetitions of the word, no account was taken in these observations of accuracy.

For the ten selected repetitions of the word the ratio of each time interval to the next is calculated. The average of the coefficients of variability of each of these ratios gives an inverse measure of the *consistency* with which the time relations within the word have been maintained. The greatest deviation from uniformity shown by the average of any one of the ratios is taken as the measure of *deviation* (which for consistent subjects is a measure of rhythmical emphasis). The greatest single deviation is for this purpose preferable to the average of all the deviations, since the latter is affected by the form of the rhythm adopted, whereas the former indicates only the degree of emphasis placed on some rhythm or other. (For instance, the average deviation in the rhythm '. . . . (*one* two three—*one*)' would be less than that in the rhythm '. . . . (one *two*—one *two*),' though the degree of emphasis might be the same in each.)

In one or two instances the scores for deviation and for consistency show a negative correlation (– 0·59 ± 0·10 for Group I 'heterogeneous'; – 0·57 ± 0·09 for Group IV 'hormone'; – 0·40 ± 0·11 for Group IV 'heterogeneous'). This serves to emphasize the fact that to some extent mere irregularity increases the subject's deviation score, and that both aspects of the performance have always to be considered: namely, the degree to which uniformity is departed from, and the consistency with which a pattern is maintained. It seems unwise to attempt any combination of these two variables into some single measure of 'the degree of rhythm present.' Such a measure would obscure important differences between subjects, and it is doubtful whether it could have any clear meaning psychologically.

Very large differences occurred between different subjects' scores, for both deviation and consistency. The lowest average deviation from uniformity was 0·03, the highest was 1·92. The lowest average coefficient of variability was 4·9, the greatest was 29·5.

VII. RHYTHM AND SPEED.

The results indicate fairly clearly a positive relation between rhythmical emphasis and capacity for speed. This fact is obscured to some extent by the presence of the third variable, consistency; and some allowance has to be made for this factor. When consistency is very low (*i.e.* when the subject has typed erratically) the positive relation between the deviation score and speed no longer appears. In the following table the correlation between deviation and speed is shown for each group, together with the correlation when erratic subjects are omitted. Those subjects were considered to type erratically whose score for variability exceeded the mean of the group plus five times its probable error.

Correlation of Deviation and Speed.

Group I, 'hormone': 0·58 ± 0·10 (omitting erratic subjects: 0·61 ± 0·11).
Group I, 'heterogeneous': 0·10 ± 0·15 (omitting erratic subjects: 0·40 ± 0·15).
Group II[1], 'hormone': 0·61 ± 0·17 (no erratic subjects).
Group II, 'heterogeneous': 0·73 ± 0·13 (no erratic subjects).
Group III, 'hormone': 0·22 ± 0·12 (omitting erratic subjects: 0·17 ± 0·14).
Group III, 'heterogeneous': 0·23 ± 0·12 (omitting erratic subjects: 0·36 ± 0·13).
Group IV, 'hormone': 0·35 ± 0·12 (omitting erratic subjects: 0·45 ± 0·12).
Group IV, 'heterogeneous': 0·31 ± 0·12 (omitting erratic subjects: 0·49 ± 0·12).

It will be seen that there is some positive correlation (though it is not always significant statistically) between speed and deviation on all groups, and that it is nearly always greater when highly erratic subjects are omitted. In order that subjects from the different groups (which had had different lengths of training) could be compared directly, each subject's score for speed and for deviation was expressed as a percentage of the mean score of her group. Erratic subjects (numbering sixteen for 'heterogeneous' and twelve for 'hormone') were excluded. For the remainder of the subjects, treated in this way as one group, the correlation between speed and deviation was, for 'hormone' 0·36 ± 0·07, and for 'heterogeneous' 0·45 ± 0·07.

The practical importance of this degree of correlation can be shown by dividing the subjects, according to their deviation scores, into a more rhythmical half and a less rhythmical half[2]. With both words the less rhythmical half is slower than the more rhythmical: for 'hormone' it takes 13·4 per cent. longer, and for 'heterogeneous' 14·4 per cent. longer. The difference between the means is for 'hormone' 13·0 ± 2·6 (5·0 times the probable error), and for 'heterogeneous' 13·35 ± 2·8 (4·7 times the probable error).

One would suppose that there must be some upward limit to the extent to which a rhythm can profitably be emphasized. The results obtained so far, however, give no clear indication that this limit was ever reached.

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[1] Correlations for Group II are shown for formal completeness; the group was of course too small for much significance to be attached to the figures.

[2] A few subjects in the middle ranks for deviation could not be assigned to either half, since identical deviation scores occurred on both sides of the median. There remained, however, for each word, fifty-six subjects who could be divided into the two classes.

It remains to consider whether the relation between rhythm and speed indicates that rhythmizing increases the capacity for speed, or is merely an accompaniment of rapid work. This question can be answered by comparing the scores of the four groups, which, owing to differences in length of training differed significantly in speed. In the following summary statement the difference between the means of the groups for deviation is expressed as a percentage of the larger mean, and the significance of the difference is stated in terms of its probable error. No significant differences in consistency occur among the groups. The figures given are for 'hormone'; those for 'heterogeneous' are practically identical.

Group I is insignificantly slower than Group II (6·5 per cent.: 1·9 times probable error) and deviates more[1] (54 per cent.: 4·8 times probable error).
Group I is faster than Group III (10 per cent.: 3·3 times probable error) and deviates more (54 per cent.: 6·0 times probable error).
Group I is faster than Group IV (21 per cent.: 6·6 times probable error) and deviates more (49 per cent.: 4·9 times probable error).
Group II is faster than Group III (16 per cent.: 5 times probable error) and deviates to the same extent.
Group II is faster than Group IV (26 per cent.: 7·8 times probable error) and deviates less but insignificantly (8·7 per cent.: less than probable error).
Group III is faster than Group IV (12 per cent.: 3·8 times probable error) and deviates less but insignificantly (8·7 per cent.: no greater than probable error).

It will be seen that no statistically significant differences in deviation occur among the three student groups, although there are marked differences in their speeds (owing to differences in the length of training). Yet among individuals of equal experience there is a positive relationship between rhythm and speed. Hence this relationship must mean that the subject's tendency to rhythmize indicates her *capacity* for speed, and does not merely *accompany* the achievement of speed.

VIII. EFFECT OF TRAINING IN UNIFORMITY.

The differences between the means of the various groups also provide evidence as to the effect of training in uniformity. All the student groups show a markedly lower deviation score than do the experienced typists who had not been trained in this way, so that it may be said that their special type of training has succeeded in checking the natural tendency to emphasize the rhythms strongly. As far as their speed is concerned this approach towards uniformity seems not to have had much effect either way. Group II (six students with a year-and-a-half's training) was slightly (insignificantly) faster than Group I, but this is not surprising since the conditions of the experiment (the repeated typing of one word) gave little opportunity for the extra experience of Group I to show itself. Had there been any advantage in adherence to uniformity, Group II might well have been faster under these conditions than Group I, and even Group III, with one year's training, might have been as fast as the experienced typists. In point of fact Group III was considerably slower. There seems, therefore, to have been no clear effect on speed in either direction through the reduction in rhythmical emphasis. On the other hand, the tendency *within* each group for speed and rhythmical emphasis to be positively related still remains.

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[1] *I.e.* departs more markedly from uniformity of time interval between successive strokes.

This suggests that capacity for speed is related to the strength of the *tendency* to rhythmize and that the function of rhythmization is fulfilled even when the rhythm is only slightly emphasized. This is to be expected if one supposes that the function of rhythm is to organize or integrate successive acts (in this case the component movements of the task). The degree of rhythmization may normally be judged by the extent to which the pattern is emphasized. But this outward expression of the integration may be deliberately checked without the integration's becoming any less real. This corresponds to the fact noted by H. E. O. James (3) that the subjective experience of rhythm, once established by objective differences in a series of sounds, will be maintained even when these differences are gradually eliminated. Training in uniformity will reduce the amount of emphasis given to the rhythms; but unless it is completely successful it will still leave individual differences, and these will correspond roughly to the relative strength of each subject's tendency to rhythmize.

IX. ERRATIC WORKERS.

Those subjects whose performance has been termed erratic (*i.e.* whose variability score exceeded the mean score of the group plus five times its probable error) include both very fast and very slow workers. The number of subjects (12 for 'hormone' and 16 for 'heterogeneous') is too small to allow any generalizations to be made with confidence. No correlation appears between deviation and speed, or between deviation and consistency. There is, however, a small positive correlation between consistency and speed among these erratic subjects, *i.e.* the less erratic they are, the better their speed. The correlations are, for 'hormone' 0·54 ± 0·14, and for 'heterogeneous' 0·35 ± 0·15. This is interesting, in that it is the reverse of the tendency found among the consistent workers, where the correlation between speed and consistency is slightly negative (for 'hormone' – 0·29 ± 0·075, for 'heterogeneous' – 0·34 ± 0·077).

It may be that these subjects set about the task in a different way from the others: that they do not integrate their movements into a muscular unit, but treat each letter stroke separately. This would imply that they have remained in what is supposed to be one of the earlier stages in the learning of typewriting and that those who attain high speed do so in spite of their method. The slight correlation between consistency and speed may mean that the faster of these workers did to some extent rhythmize their movements and so benefited. It is to be expected that among extremely erratic workers any approach towards rhythmization will show itself in lessened inconsistency, since in the absence of some minimum degree of consistency, deviation from uniformity cannot be regarded as rhythmization. Evidence from a further experiment (see XI below) suggests that erratic workers benefit from having a rhythm prescribed for them. Thus it may be supposed that rhythmization would be of value to these workers, as to others, but that certain of them succeed in compensating for their lack of rhythm by some other factor.

Much closer study of the erratic workers would be necessary before anything more definite could be said.

X. EXPERIMENT IN INDICATING RHYTHMS TO LEARNERS.

Following the observations of rhythms produced naturally by typists, an experiment was carried out on the effect of the learner's deliberately adopting a rhythm at the beginning of practice on a new word. The experiment consisted in the comparison of two different methods of word-drill (*i.e.* the continuous repetition of one word in order to attain automaticity). The first method was that of adhering as far as possible to uniform intervals between successive strokes; the second method that of adopting a rhythm or pattern of movements, different for each word, and indicated before practice began.

The subjects of the experiment were twenty-one students who had been learning typewriting for two terms and were all familiar with the keyboard. About half the class had been urged throughout their training to keep to uniform time intervals between successive strokes. The other half had been allowed to type in whatever rhythm they preferred, but since they worked in the same room and overheard instructions to the first half, most of them reported that they had been instructed to maintain uniformity of interval. No difference in efficiency was apparent between the two groups.

Fourteen words were selected for practice and were divided into two sets of approximately equal difficulty, as follows:

Set A: reduced, possible, process, unlikely, necessity, inquiries, enclosed.

Set B: decided, received, furnish, problems, practical, attention, position.

The students also were divided into two groups, care being taken to make the groups roughly equal in efficiency and to include in each an equal number of those who had been urged to maintain uniformity of interval. Group I practised Set A words uniformly and Set B rhythmically; Group II practised Set A rhythmically and Set B uniformly. The results show that there was no significant difference either in the two sets of words or in the proficiency of the two groups of students.

Each word was practised repeatedly for a minute and a half, a brief pause was made, and the word was again typed repeatedly for one minute, this latter period being called the 'test.' The uniform method was tried first, with both groups of subjects in the same room. A week later the two groups worked apart from each other and the rhythmical method was used. With the uniform method the subjects were urged, before each period of practice and each test, to maintain complete uniformity of time interval between successive strokes; and before every other word they listened for a short time to the regular ticking of a metronome set at various speeds. With the rhythmical method the subjects listened, before each word, to a rhythm reproduced on a buzzer, and were instructed to adopt as far as possible that rhythm in their strokes. They were told that they might change the rhythm slightly if it seemed unsuitable to their fingers. (Only two students reported having made changes and those they made were slight.)

The device for reproducing the rhythm consisted of a gramophone motor, working a brass turn-table on which paper discs could be fixed. Each disc was punched with holes in a prescribed pattern, and a wire brush pressing against the disc completed an electric circuit at each hole. A small buzzer placed in the circuit reproduced audibly the pattern on the disc.

The rhythm to be adopted for each word was decided upon chiefly on the principle that the interval between strokes made with fingers of the same hand is longer than that between strokes made with alternate hands. This general principle, however, was always checked by examining the rhythm actually adopted by several experienced typists.

A 'net speed' for each subject was calculated. This consisted in the average number of strokes made in the one-minute test, minus fifty strokes for each mistake. Only one mistake was counted in any one word. The penalty for mistakes is of course quite arbitrary, but it represents the standard adopted for international typewriting contests.

XI. Results of the experiment.

Taken by themselves the changes in net speed which followed the introduction of rhythm appear extremely inconclusive. They become far more significant, however, in the light of results obtained from a rhythm test which was given to the subjects some months before the experiment.

The effects of the prescribed rhythms on the subject's performance may be stated briefly. Twelve subjects had a higher net speed with rhythm than with uniformity, and nine subjects a lower net speed. The average increase in speed of those subjects who improved with rhythm was 38 per cent.; the average decrease of those whose speed dropped was 41 per cent. Subjects varied, as the following summary shows, in the relative effect of the rhythm on their gross speed and on their accuracy:

Increase in gross speed and in accuracy: 6 subjects.
Increase in gross speed, decrease in accuracy: 7 subjects.
Increase in gross speed, accuracy unaffected: 1 subject.
Decrease in gross speed and in accuracy: 4 subjects.
Decrease in gross speed, increase in accuracy: 2 subjects.
Decrease in speed, accuracy unaffected: 1 subject.

These results are insufficient to justify any generalization concerning the effects of the rhythms on accuracy. In a short test, chance alone would account for fairly large variations. Of those whose accuracy varied markedly (the number of errors decreasing or increasing by more than 20 per cent.), seven subjects increased in accuracy and seven decreased.

The preferences and opinions of the students themselves were obtained in the form of answers to a questionnaire. This yielded little information since the subjects were unused to introspecting. The most striking feature of the answers was the complete lack of correspondence between the method preferred and the method that produced the better results. Eighteen students answered the questions, ten preferring the rhythmical method and eight the method of uniformity. Of the ten preferring rhythm, six did better with rhythm and four with uniformity; of the eight preferring uniformity, three did better with uniformity and five with rhythm.

The results of the rhythm test already mentioned throw rather more light on the differences in the effects produced by the prescribed rhythms. This test, which is not yet standardized, consisted in the subject's striking a number of small targets as rapidly as possible with a metal stylus. The test gave scores for deviation and consistency on the same principle as those obtained from the typing observations. In this test four subjects proved to be erratic (their score for variability exceeding the mean of the group plus five times its probable error). In the experiment these four subjects all gained in net speed when rhythms were introduced. This was probably due to the steadying effect of having a rhythm prescribed for them; normally they would never have settled to any one rhythm. Five students rhythmized very little (their deviation score being less than the mean of the group minus five times its probable error), and all these students lost in net speed when rhythm was introduced. It seems probable that they found difficulty in introducing the rhythms, and that the effort of attempting it distracted them and disturbed their performance. Only one, however, reported any difficulty. Possibly they would have done better if they had listened longer to the indicated rhythm before starting work. But whether people who are naturally poor at rhythmizing can under any conditions benefit from having a rhythm prescribed needs to be decided by further experiment. Five subjects rhythmized markedly in the test, and of these, three increased in speed with the introduction of rhythm and two decreased. Seven subjects made an average score in the test; in the experiment five increased in speed with rhythm and two decreased. No relation appears between the subjects' preferences as expressed in the questionnaire and their performance in the rhythm test.

With so small a number of subjects for both test and experiment, only the general trend of the results can be considered. This seems to be that a suitable rhythm of work, indicated before practice begins, is valuable to the majority of learners, and that most of those to whom it may be detrimental can be discovered by a preliminary test. More precise results could probably be obtained with an improved rhythm test and improved methods of indicating rhythms.

XII. SUMMARY.

Studies of typewriting show that there is a general tendency for typists to form time patterns or rhythms out of their key-strokes. A positive relation exists between the degree to which this rhythmization is stressed and the subject's capacity for rapid work. If the subjects are divided into a more rhythmical and a less rhythmical half, the former is about 13 per cent. faster than the latter (training and experience being equal). The relation between rhythm and capacity for speed does not hold good for erratic workers, who, though not rhythmical, may type very rapidly.

The speed of most learners may be increased if a suitable rhythm is indicated for each word before practice begins. The speed of some subjects, however, may suffer through the attempt to adhere to a prescribed rhythm. Most of these subjects show themselves in a rhythm test to be poor at rhythmizing.

I wish to express my thanks to Dr C. S. Myers for his guidance and criticism throughout my work. I am also indebted to the Controller of the Money Order Department of the Post Office, to the authorities of the City of London College, and to Mr A. C. Marshall, lecturer at the College, for granting facilities for experimental work.

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THE RELATIONSHIP BETWEEN GENERAL MENTAL DEVELOPMENT AND MANUAL DEXTERITY[1].

By PORTIA HOLMAN.

(*From the Psychological Laboratory, the University of St Andrews.*)

I. *Description of investigation* (p. 279).
II. *Results* (pp. 280–2).
III. *Conclusions* (pp. 282–3).

I. DESCRIPTION OF INVESTIGATION.

A SIMPLE test of hand and eye co-ordination known as the Ball-and-Slot test has been used as an index of manual dexterity. The apparatus consists of a small glass-covered box which the subject holds in front of him in his left hand. The subject is required to insert a small steel ball and let it rest on a small platform and then tip the box with a single movement so that the ball goes into a specified slot at the bottom. Subjects have four practice periods a week for four weeks. Each practice period consists of 200 shots and the score is the number of balls correctly placed.

General mental development has been measured by the Binet-Simon (Burt Revision) Scale and the Mental Ratio of each child obtained.

The average age of the children tested was 13 years. Fig. 1 shows the respective practice curves of:

Group A: 18 children from a Scottish secondary school with average mental ratio 103.

Group B: 33 children from a school for the mentally defective (feeble-minded) with average mental ratio 58.

Both groups worked under the same general conditions and both were paid a weekly pocket money of 4*d.* intended to ensure regular attendance.

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[1] The work on which the following results are based has been done with the aid of a grant from the Industrial Health Research Board.

II. RESULTS.

Tables I and II indicate the differences between the two groups during the practice period.

Table I. *Mean scores in Ball-and-Slot test on first day of each week and last day.*

Day	1	5	9	13	16
Normals	114·5	151	163	169	178
Defectives	82	138	155	164	174
Difference	32·5	13	8	5	4
Standard error of difference	8·0	8·9	7·1	6·9	6·9

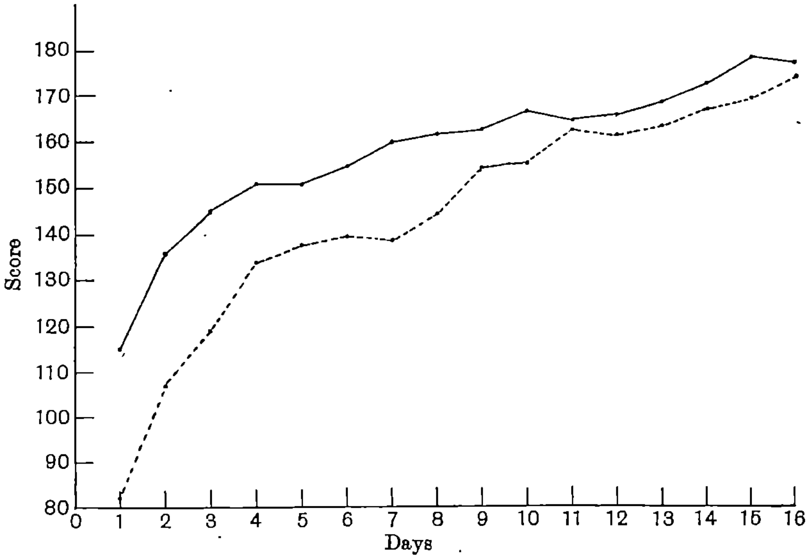

Fig. 1. Practice curves of Group A, 18 normal subjects (M.R. 103) ———
Group B, 33 mentally defective subjects (M.R. 58) - - - - -

If instead of taking scores on successive Mondays as representative, we take the average for each week, we may express the result as follows:

Table II. *Weekly average scores in Ball-and-Slot test.*

Week	1	2	3	4
Normals	144	164	170	173
Defectives	114	146	167	173
Difference	30	18	3	0
Standard error of difference	8·0	8·0	7·0	6·5

It is possible further to investigate these differences (1) by taking out of the normal group the seven worst whose mean score on the first day is 82, precisely the same as that of the M.D. group, and (2) by taking out of the M.D. group the eight best whose mean score on the first day is almost identical with that of the normal group. (N.B. There is no significant difference between the mental ratios of the best and worst normals in the Ball-and-Slot test or of the best and worst defectives.)

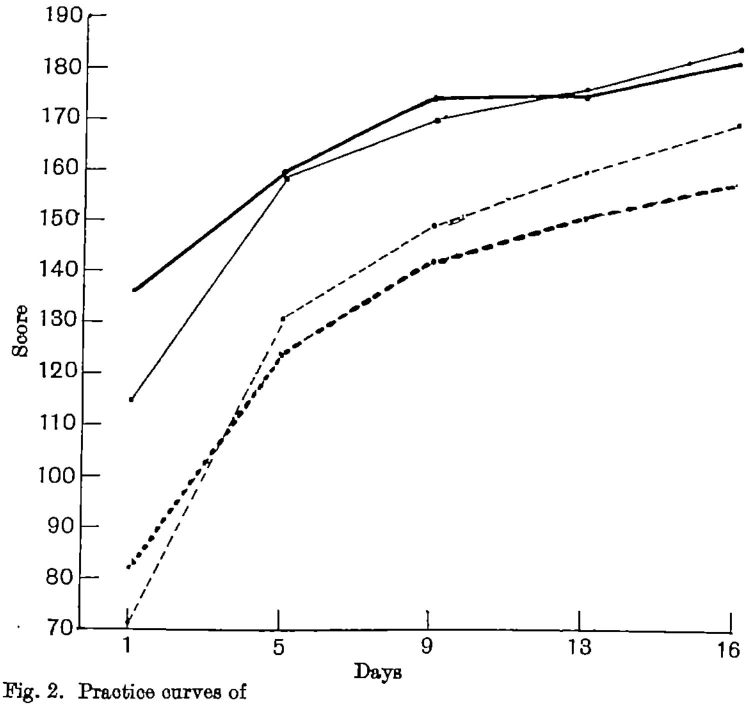

Fig. 2. Practice curves of

11 best normals ———
7 worst normals - - - - -
8 best defectives ▬▬▬
25 worst defectives ▪▪▪▪▪

Tables III and IV give the comparison of their performances.

Table III. *Mean scores on first day of each week and last day.*

Day	1	5	9	13	16
Normals (7 lowest)	82	124	143	152	(162)*
Defectives	82	138	155	164	174
Difference	0	– 14	– 12	– 12	(– 12)
Standard error of difference	9·9	11·3	11·3	8·5	(18·0)

* 3 subjects only.

Table IV. *Mean scores on first day of each week and last day.*

Day … … …	1	5	9	13	16
Normals	115	151	163	169	178
Defectives (8 best)	115	159	171	177	186
Difference	0	− 8	− 8	− 8	− 8
Standard error of difference	8·0	9·0	6·5	6·1	7·0

Fig. 2 shows practice curves of four sub-groups:

(*a*) 11 best normals in Ball-and-Slot test.
(*b*) 7 worst normals in Ball-and-Slot test.
(*c*) 8 best defectives in Ball-and-Slot test.
(*d*) 25 worst defectives in Ball-and-Slot test.

The facts shown by these figures are:

1. That defectives started in this test of hand and eye co-ordination very much worse than normals. (Difference between mean scores = 4 times S.E.)

2. (*a*) That at the end of three weeks' practice there is practically no difference between defectives and normals in the test.
   (*b*) That the difference between the groups is reduced to an amount not statistically significant after four days.

3. (*a*) Those defectives who start as well as the normal group reach a higher final score than the normal group.
   (*b*) Those normals who start as badly as defectives do not reach as high a final score as the defective group.

III. CONCLUSIONS.

We may perhaps generalize that on the first day of performing a kinaesthetic test there will be a marked positive association between that ability and 'intelligence,' but the association will be smaller on all subsequent occasions until at the end of a few practice periods the two abilities will be completely independent.

This points to the conclusion that the relation between general mental development and the factor, whether specific or general, measured by a test such as the Ball-and-Slot test may be more complex than the ordinary statement of the two-factor theory admits.

The practical value of these results is still tentative. They are in need of confirmation in actual industrial conditions but they at least suggest applications of some importance to those concerned with the training and employment of defectives.

They have further a practical bearing on the use that is made of the results of the tests of kinaesthetic abilities given on one occasion only. It appears that where differences in intelligence are negligible, differences in first performances are probably a reliable indication of subsequent differences (*i.e.* the reliability of a test such as this is high), but where differences in intelligence are marked, differences in the kinaesthetic test results will be associated with differences in intelligence, and a correction should always be made for the temporary influence of 'general intelligence,' an influence which, at least in a simple operation such as this, is obliterated at the end of a relatively small number of practices.

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KINAESTHETIC FACTORS IN THE RECALL OF MUSICAL EXPERIENCE.

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I. *Introductory. The two problems* (pp. 284–6).
II. *The factor of kinaesthetic habit sequence in the reproduction of music* (pp. 286–91).
  (*a*) *Vocal music* (pp. 286–9).
  (*b*) *Instrumental music* (pp. 289–91).
III. *The factor of kinaesthetic association in the recall of musical experience. Form of investigation* (pp. 291–4).
IV. *Analysis of the introspections* (pp. 294–9).
V. *Statistical analysis* (pp. 299–301).
VI. *Summary of results and conclusions* (p. 301).
References (pp. 301–2).
Appendix. *Tests employed in the investigation* (pp. 302–7).

I. Introductory. The two problems.

The conclusion arrived at in 1922 by Otto Ortmann(1), that it is impossible to base an adequate psychology of music appreciation on purely auditory grounds, is true, even if the physical and psycho-physical or emotional elements involved in the affective reaction to musical stimuli are excluded. Musical activity can consist only in three forms of human behaviour: firstly, in the process of 'thinking in music'; secondly, in the production or the control of the production of actual musical sounds; and thirdly, in the conscious reception of music.

'Thinking in music' may vary from the recall of some simple musical experience to the complex processes involved in the composition of a symphony. However simple or complex the process may be, it must consist essentially in the recurrence of some form of musical experience, and such recurrence may vary from the preservation of some isolated auditory fragment to the recall of some part of an organized body of musical knowledge, available either as a basis for the appreciative judgment of new musical experience, or for the eduction of new correlates for the creation of new musical work.

An original musical experience must consist either in the actual production or control of the production of music, or in listening to music, and while the former experience obviously includes a great number of processes which are kinaesthetic and visual, the latter experience might possibly include visual factors and the recurrence of previous relevant motor experience.

Although it is not as usual to speak of 'remembering' motor activities in quite the same way as one speaks of the recall of visual, auditory or other cognitive events, yet the formation of motor habits may be regarded equally as a manifestation of the Laws of Retentivity and Association (2), which are equally true if worded: "Any kinaesthetic event, by occurring, tends to establish a disposition to occur again"; and "a sequence of kinaesthetic events, by occurring as a sequence, tends to establish a disposition to recur as a sequence." The process of traversing, 'by rote,' the same kinaesthetic path until it has been trodden into a 'habit sequence,' forms so much of human and animal behaviour that the Watsonian Behaviourists, arguing from the particular to the general, base all human behaviour on this tendency (3). Obviously true of such activities as putting on one's clothes, or in the complex process of driving correctly from a golf tee, it is hardly less obviously true of such mechanized processes as the learning of poetry or prose by constant repetition. The extent to which the tendency to form kinaesthetic habit sequences is a factor in the recall of musical experience forms the first problem of this enquiry.

There is, however, a second way in which the Law of Association, as applied to motor behaviour, may be stated: "A kinaesthetic event and a cognitive event, by occurring together, tend to do so afterwards." Physical behaviour, by its very nature, must tend to become associated with some change in the environment, and when it is definitely manipulative, must tend to become associated with some reaction. The closeness of the association between the normal response to some frequently performed physical act and the act itself, is best demonstrated by the shock which inevitably accompanies the absence of the expected response, or which follows some new and strange reaction. An actual experience in musical behaviour will suggest the relevancy of this tendency to the present enquiry. The present writer, as a child of eight, had already for some years studied pianoforte playing, but his experience had been limited to a few pianos, and presumably, from the ultimate event, all of the same pitch. On one occasion he was invited to play when visiting a certain house for the first time. He still recalls his shock and amazement on finding that the result of his striking the first notes, produced, not as he expected, the chord of G Major, but that of A♭. To the horror of his parents he had to abandon the 'recital.' This phenomenon of 'absolute pitch' is, or was at least on that occasion, nothing more than the acquired association of a fixed auditory response to a given motor stimulus.

The second problem is the extent to which ability to recall musical experience, in the sense of ability to think in terms of sounds in musical relation, is influenced by the recurrence of previous kinaesthetic experience in the production of similar sounds and sound relations.

Of these two problems, the first is the more easily investigated, as it applies only to the formation of habit sequences in the actual reproduction of music, while conclusions as to the existence or non-existence of the tendency are deducible from generally observable data. The second problem demands investigation under controlled laboratory conditions.

II. THE FACTOR OF KINAESTHETIC HABIT SEQUENCE IN THE REPRODUCTION OF MUSIC.

There are two forms of music, vocal and instrumental, but the differences between the processes involved in the production of either are more apparent than real; for the voice is but an instrument controlled by physical forces equivalent to the physical manipulation of levers and strings involved in the 'playing' of artificial devices for the production of controlled sounds. In each case, the process of 'memorization' consists in the acquisition of the ability to reproduce the series of physical movements necessary for the reproduction of the series and groups of sounds presumed to have been 'memorized.' The problem is the extent to which this apparent memorization is kinaesthetic.

(*a*) *Vocal music.* In the production of vocal music, the necessary motor co-ordination is so much more generally, consistently and earlier practised, especially in ordinary speech, than is the case in the production of instrumental music, that it tends to become mechanized at a very early age, and then to be forgotten, so that no conscious control of the laryngeal and lingual processes is necessary or desirable in the expression of vocal sounds. On the contrary, any consecutive fluency in speech or song obviously necessitates that the complex motor processes involved in the utterance shall have become so mechanized that the whole of the mental energy shall be available for the thought to be expressed. It is only when some new sound has to be produced, such as a French '*u*,' that the motor processes involved once more demand conscious guidance. One of the most curious and interesting examples of the conscious effort to control the production of vocal sounds by the deliberate modification of physical behaviour is the conventional instruction given to students by teachers of singing. The pupil is instructed "to bring the notes more forward," "to produce the notes more in the head" or "less in the nose," "to avoid tightening the muscles of the neck," or "to lower the head and widen the mouth as in laughing to produce a high, soft note." It is obvious that the effective rendering of any vocal work would be seriously imperilled if not quite impossible until the various muscular contortions regarded as essential to the production have become as mechanized as those in ordinary speech.

Vocal music consists of a series of single sounds in musical relation, and demands, therefore, for its production, a *series* of motor processes. It is generally accepted, and demonstrable from general human experience, that the repeated performance of any kinaesthetic series tends to establish a disposition for the whole series to become mechanized as a motor habit sequence, and the same tendency is demonstrable as being true of the production of vocal music. The detestable but unavoidable tendency to continue to go over and over again some tune or section of a tune, once the process has been stimulated, to traverse the mechanized route *ad nauseam*, not only without conscious direction, but in opposition to the desire, is a generally known experience, popularly known as "having a tune on the brain."

The following experiment was performed to investigate the extent to which this kind of music 'memorization' consists in, or depends on, the establishment of kinaesthetic habit sequences. A group of fifty school children and one of forty adults were asked: "How many notes at the end of 'God Save the King' go in the same direction?" The subjects were then asked: "In order to answer the question, did you hear the tune in your mind, or did you have to sing it to yourself, or what happened?" Three of the adults answered that they had "played the tune" to themselves; all the children and the rest of the adults had "sung the tune to themselves." There is little doubt that anyone who was asked such a question for the first time, that is, who did not already know, would have to adopt some such kinaesthetic procedure, rather than try to recall some previous auditory experience of the tune. The relative value of auditory images and recurrent kinaesthetic experience as factors in musical thought is more fully investigated in Section III.

While it is undeniable that motor habit sequences do play a great part in the 'recall' of vocal musical experience, it would be fallacious to assume, as do the Behaviourists (3), that all musical recall, or even all vocal musical recall, is, therefore, of this kind. In the memorization of vocal work, one must assume the possibility of reproducing in actual sounds some purely auditory experience; to sing immediately a given passage which has only been heard, as it is possible to repeat a passage of prose or poetry after it has been heard. Important factors controlling the ability in the case of the prose or poetry would be the length of the passage, the number of times it was heard, and the intelligibility of the passage[1]. The ability to reproduce a single given sound seems the irreducible minimum of any form of vocal ability, and itself consists in the physical reproduction of an auditory experience. That a short melody is similarly capable of immediate reproduction is assumed as the basis of the method of teaching songs to classes of children, as recommended by the Board of Education[2]. The experimental investigation described in Section III, however, shows how limited is the ability to recall accurately any but the shortest and simplest of passages which are only presented auditorily. Owing to the general tendency of a singer to associate immediately and unconsciously the requisite physical movement with a given sound, as in ordinary speech, it is difficult to compare experimentally the relative efficiencies of auditory and kinaesthetic methods of learning new vocal material. Rather from the interest of the introspections than from any scientific value that the experiment had, the following account of a tentative experiment on these lines is inserted.

A professional singer with some introspective experience was asked to listen to a tune, and as far as possible to avoid singing it to herself. She was told that she would be asked, after each hearing of the tune, if she could remember it well enough to sing it, and to make any relevant introspective comment. Later, a similar tune was presented, and she sang it, after it had once been played through, at each subsequent presentation. The two tunes, No. 330[3] and No. 349[4], second tune, from *Songs of Praise*, 1931 edition, were chosen as having the same metre, similar rhythm, and about the same number of notes. To eliminate the visual factor, the subject was not allowed to see the score. The following is the record of the experiment.

Subject. (After the first presentation of the tune.) "I haven't the slightest idea of it."

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[1] *Vide* experiments by Prof. C. W. Valentine (4).

[2] "Until adequate proficiency in reading is obtained, the learning of songs should be mainly by ear" (5).

[3] "Diva Servatrix," Bayeaux Church Melody.

[4] "Iste Confessor," Rouen Church Melody.

(After the second.) "One part of the tune seemed unusual, rather extraordinary; I find it very difficult not to sing to myself as you play the tune; this involves real definite effort; I couldn't possibly sing it yet."

(After the third.) "I know how it starts, now, and I didn't notice the extraordinary part so much; I think one part of the tune is repeated." (N.B. No phrase in the tune occurs twice.)

(After the fourth.) "I couldn't sing it through even now. I know how it finishes."

As the tune was begun for the fifth time, she laughed; at the end of the fifth repetition she tried to sing it, but was herself astonished at the completeness of her failure. She sang the first two bars accurately, but with some hesitation, then paused; she sang the last four bars, the 10th to the 14th, accurately and with confidence; she then reproduced inaccurately the 7th and 8th bars, but correctly reproduced the cadence which formed the 8th bar. She made the following interesting comment: "You don't know whether you know it or not until you try to sing it."

(After the sixth.) As before, the first two bars were followed by the last four, sung even more definitely than before.

(After the seventh.) The same six bars were again sung, and the testee said, in desperation: "I can't learn it."

After the preliminary presentation of the second tune, the testee said: "That tune is more difficult than the other." After singing it through twice as it was played, she sang it through alone with two prompts, and then repeated the whole tune correctly.

On the following day, she was asked how much she could recall of either tune. She again recalled and connected the first two and last four bars of the first tune, but sang through the second tune accurately, thus recalling in each case the parts she had sung on the previous day.

(*b*) *Instrumental music.* More definite light is thrown on the problem by the investigation of the processes and methods of memorizing instrumental music. The reproduction of instrumental music may take two entirely distinct forms: firstly, it may consist in the reproduction, on some instrument, of the gist of some recalled auditory experience, such as the melody of a song, or some simple melodic work; or secondly, it may consist in the accurate memorization of a musical work. The distinction between the two processes can best be made by making further use of the verbal analogy. The first is essentially equivalent to the ability to re-tell a story or recount an experience "in one's own words." It involves the ability to "think in the language." As in music it involves the existence of sufficient experience in the actual production of musically related sounds to have established immediate and mechanically functioning associations between the sounds recalled and the kinaesthetic processes involved in their reproduction, the processes involved in this form of the reproduction of music are those discussed in Section III of this paper.

The second process is essentially equivalent to the accurate recitation of some literary passage which has been deliberately and consciously memorized. While it may be possible to reproduce vocally, or on some other melodic instrument, some simple auditory musical experience, the accurate reproduction of anything but the simplest instrumental music, from auditory experience alone, is literally impossible. The factor of technical difficulty, present in the production of music even from those instruments capable of producing only one note at a time, must necessitate that the 'memorization' of such music shall be the recurrence of the actual production of the passage. Any but the most extremely simple instrumental music can only be 'memorized' by the frequent repetition of the actual production of it; *i.e.* by the formation of a kinaesthetic habit sequence. As a corollary to the law of motor association, that the occurrence of a sequence of motor events tends to establish a disposition for the recurrence of the sequence, so the formation of any such mechanized habit sequence demands the frequent performance of the sequence for its establishment. The 'memorized' reproduction of a complex musical work, consisting as it does, in the 'memorized' performance of a highly complex series of physical movements, must at least have included, as a factor in the process in the acquisition of the achievement, the frequent repetition of the whole series involved. The reliance placed on this kinaesthetic factor may be illustrated in many ways from the experience of any student of instrumental music: by the difficulty generally experienced in the eradication of errors which have become mechanized by constant repetition; by the difficulty generally experienced in beginning a passage at an unusual place; by the tendency of a 'prompt' at the beginning of a 'forgotten' passage to stimulate the recurrence of the series, or by the difficulty of changing an acquired method of 'fingering' a given passage, for another method of producing the same passage.

The following extracts from a standard work on pianoforte technique (6), selected at random from a group of similar works, further illustrate the importance, in practical musicianship, of the formation of motor habits, which can be left to function without conscious direction:

> In degree, as there is consciousness of physical means during a performance, by so much will a performance fall short of its best....All conscious thought is to be directed to the imagination of sound; the physical efforts necessary to materialize these imaginings must be a subconscious muscular response to the desire of the mind....Practice requires conscious attention to the best means and physical conditions, leading gradually to the most complete subconscious reproduction of these various physical sensations during performance.

It would again be fallacious to assume that, because so much of the 'memorization' of musical works is the acquisition of kinaesthetic habit sequences, therefore the whole process is kinaesthetic, or that the performance of a great work can be nothing more than the mechanized antics of a physico-chemically reacting robot. There are also the auditory factor, presumably strengthened by the repetition of the auditory experience which must accompany each 'practice' of the work, the visual factor of the printed score, and, in some cases, the assistance possibly afforded by the ability to make a structural or harmonic analysis of the work.

It can, however, be safely concluded, that the act of reproducing a 'memorized' musical work consists essentially in the acquisition of a mechanized kinaesthetic habit sequence, and that any reliance on purely auditory experience would be so restricted as to be almost negligible in practice.

III. THE FACTOR OF KINAESTHETIC ASSOCIATION IN THE RECALL OF MUSICAL EXPERIENCE.

The second problem is the extent to which ability to recall musical experience, in the sense of ability to think in terms of sounds in musical relation, is influenced by the recurrence of previous kinaesthetic experience in the production of similar sounds or sound relations. It is self-evident that the ability to reproduce, vocally or instrumentally, any given sound not only involves, but actually consists in, the immediate association of that particular sound with a previous motor experience; the performer must know which particular event will produce the given sound, or recall the sound which will result from a given motor stimulus. "Those who, while singing, trust for guidance to the ear only, should be reminded that the ear, perceiving the sound after its production, is merely able to judge of its effect, and cannot possibly influence its cause (7)." This truth is even more obvious when applied to instrumental music.

The existence of any such associations assumes the existence of previous experience in the actual production of sounds in musical relation, experience capable of almost infinite individual variation. The ability to produce vocally sounds in definite pitch relation is, to a limited degree, fairly general. Any experience in the production of harmony must necessarily be limited to the 'players' of those instruments capable of producing simultaneously more than one sound, or to the control, or conducting, of a group of singers or instrumentalists. The ability to produce sounds in temporal relation, *i.e.* to produce rhythmic patterns, is fairly generally practised in a multitude of forms, varying from the vague ambling characteristic of the modern ballroom *habitués*, to the complex tympanic orgies of the Sarawak Malays. To obtain evidence of the influence of this kinaesthetic association on the ability to recall sounds in definite musical relation, the following experiments were devised.

Form of investigation. The first experiment comprised a group of three tests, partly objective and partly subjective, which were submitted firstly to a group of fifty school children, whose ages ranged from 10 to 14 years. The second experiment consisted in the resubmission of the tests in a modified and extended form, firstly to a small group of six trained psychologists from the Birmingham University Psychological Laboratory, and secondly to a group of forty students of the Dudley Training College. In all the tests, the questions were asked orally, and the replies were written by the subjects. The tests employed in Experiment I were:

(*a*) a musical recall test, partly objective, partly subjective;
(*b*) a group of introspective, non-musical imagery tests;
(*c*) a group of intelligence tests for correlation purposes.

(*a*) The *musical recall test* was an extension of that employed by the writer in a previous investigation (8), with the addition of a number of introspective questions. The subjects were asked to listen to a tune which was either played on a piano, violin or flute, or sung. After the experience of listening to each tune, the testees were asked to write answers to a number of questions involving some analysis of the recalled experience, the questions being unexpected and varied. The testees were then asked the following questions:

(1) At the time you heard the tunes, did you notice sufficient facts about them to enable you to answer the questions, or did you have to recall the tune in order to answer them?

(2) When you recalled the tune, did you "hear it in your mind," clearly, vaguely, or not at all?

(3) Did you hear it, if at all, as a mere tune, or did you hear it as a piano tune, a violin tune, a whistled tune, or a sung tune?

(4) Was it easier to recall the tune when it was played on one instrument or sung, than when it was played on others? If so, which?

(5) Did you recall one part of the tune more easily than the rest? If so, which part or parts?

(6) Did you sing the tune to yourself?

(7) Were you helped to recall the tune by recalling the sight of me playing it or singing it?

(*b*) The *imagery tests* consisted of a group of three non-musical introspective tests, referring to auditory, visual and motor experience respectively. The subjects were asked to state whether they could recall a certain sensory or perceptual experience, clearly, vaguely, or not at all. In certain cases they were asked questions to test the completeness or accuracy, or genuine nature of the recalled experience. These tests related to experiences which were likely to have happened to every subject so frequently as to have established marked tendencies to recur. They were, moreover, experiences of a simple nature, in the sense of having relatively few relevant and essential characters, as far as possible limited for their original perception to a single sense. The object was to give every facility to the formation of 'clear images,' and, by evaluating the assumed ability to recall such perceptual experiences, to compare these values with the measures of the ability to analyse recalled musical experience.

(*c*) The *intelligence tests* used were the Opposites Test, the Classification Test, and the Completion of Numerical Series Test.

For the second experiment two tests were devised. The material of the first test comprised a number of 'nonsense' tunes, of three, four, five or seven notes, all of equal duration so as to avoid rhythmic pattern, and, as far as possible, avoiding conventional melodic sequence, or suggested harmonic relation. After each tune had been played, each time on a piano, the testees were simply asked to recall the tune, and write down any relevant introspections as to the character, limitation or process of recall.

The reasons for the inclusion of this experiment were: (1) the frequent references in the introspections of the first group of subjects to the inability to recall *long* tunes, although the longest example was a rhythmic example of nine notes only; (2) the suggestion that the rhythmic pattern of the tunes was recallable when divorced from the pitch of the notes.

The second test employed a number of chords and arpeggios. Questions involving careful introspection followed the playing of each example. Typical questions were: "How many notes were there in the chord?" "Can you hear them now?" "Do you hear them, if at all, as a chord, or do you hear the notes separately?" "Can you still hear the example I played last?" "Does any note stand out more clearly than others, if so, which?" "Do you employ any form of 'motor image'?" "Can you hear the notes of a chord without motor assistance?" "Write down any further introspective findings." This experiment was performed only with the adult groups.

Four specially constructed tunes were employed in Test I, Experiment I, on its submission to the adult group of Dudley Training College students. Of these tunes, the first and third differed only in two particulars; the fourth tune was a diatonic variation of the second.

The complete tests are given in the Appendix.

IV. ANALYSIS OF THE INTROSPECTIONS.

(*a*) *The children's group.* To the first introspective question, asking if it had been found necessary to recall the tunes in order to answer the questions, or whether sufficient facts had been noticed at the time of the experience, every testee in the children's group replied, on the second submission of the test, that it had been necessary to try to recall the tune. Nine of the fifty, when the test was first submitted, replied that they had "noticed when it was played."

To the second question, asking whether the tune had been recalled 'clearly,' 'vaguely,' or 'not at all,' the replies were as follows:

37 replied 'clearly' in the first test, 'clearly' in the second;
5 „ 'clearly' „ 'vaguely' „
7 „ 'vaguely' „ 'vaguely' „
1 „ 'vaguely' „ 'clearly' „

These replies were qualified in twelve cases in four ways:

4 replied that "some were clearer than others";
6 replied that they could not recall the long ones;
1 replied that "only parts were remembered"; and
1 found it easier if she closed her eyes.

An analysis of the replies to the third, fifth and sixth questions throws an interesting light on the nature of these assumed clear 'images.' Of the hundred replies to question No. 3, fifty-five stated that the tunes were recalled not as piano, violin or whistled tunes, but as 'mere' tunes, while 33 per cent. stated that the tunes were recalled as sung tunes. As the auditory element in the initial experience consisted in the hearing of a tune played by one or other of those three instruments, or sung, the recall of the tune as a 'mere' tune, divorced from the medium in which it had been heard, suggests that this recall was not a recurrence of that element in the experience at all, that it was not an auditory image of the experience. In reply to the sixth question, "Did you sing the tune to yourself?" every testee answered affirmatively. The deduction is suggested that the testee does not rely on any genuine auditory image, but translates the experience into more easily recurrable motor impulses, which would account for the tune's being heard as a 'mere' tune, or as a sung tune, and so rarely as a violin, piano or whistled tune.

To the question: "Did you usually recall one part of the tune more easily than the rest, and if so, which part or parts?" only six testees claimed to have recalled the whole tune, 'quite clearly' being added in one case; but of these four achieved the bottom four places in the ranking. 42 per cent. claimed, on each paper, to recall the beginning most easily; 10 per cent. best recalled the end; and, excluding the six testees already mentioned, the rest wavered in the two papers between the beginning and the end.

92 per cent. found it helpful to recall the appearance of the performer; the others were undecided, but no one twice denied the suggestion.

No consistent or very satisfactory replies were given to the other question, No. 24, asking whether it was easier to recall the tune when it had been presented on one instrument than another. Almost every testee gave alternative possibilities on one paper, only to alter his decision when replying to the same question on the resubmission of the test; but this very indecision is illuminating, in that it suggests still further evidence that the medium in which the tune is presented is rapidly forgotten, if not completely ignored, and the tune recalled by motor processes as a 'mere' tune.

(*b*) *The adult group.* The analysis of the replies both of the trained psychologists and of the forty adult students reveals the same inadequacy of any auditory imagery, and the same tendency to translate the auditory experience into kinaesthetic processes. All the testees found it necessary to try to recall the tune in order to answer the questions. The nature of the recalled experience was judged to be as follows:

2 professed to have had clear auditory images;
7 „ „ 'fairly clear' images;
8 „ „ vague or very vague images;
22 had 'sung or played the tunes to themselves'; of these, 10 had had vague images, 12 had experienced no auditory image.

Of the two who claimed to have had clear auditory images, E. C. had only succeeded in answering five of the seventeen analytical questions correctly, and in answer to the question "Did you employ any form of motor 'image'?" replied "Yes, manual." E. S. N. achieved the lowest score but one, answering four only of the questions correctly.

By evaluating the assumed reliance on clear images, and correlating with the score gained for accuracy of the analysis of the recalled experience, the result was the negligible coefficient of $-0{\cdot}07$.

In reply to the questions: "Did any part of the tune linger for any time as an auditory image?" and "Did you usually recall one part of the tune more easily than the rest, and, if so, which part or parts?"

36, *i.e.* 90 per cent., limited any definite perceptual recurrence either to the beginning or the end of the tunes;
1 recalled best the 'middle';
1 said that nothing remained as an image for more than half a minute;
2 recalled as an image only the "slight variation produced by the quicker notes."
No testee claimed to recall the whole tunes or a whole tune.

In answer to the question: "Did you recall the tunes as 'mere' tunes, or as 'sung' or 'played' tunes?"

18, *i.e.* 45 per cent., recalled the tunes as 'mere' tunes;
7 recalled them as 'sung' tunes;
1 recalled them "as music on a mental staff";
1, A. M. D., answered quite definitely: "The piano tunes seemed to be translated into sung tunes."

In answer to the questions: "Can you hear any of the tunes or parts of the tunes now?" and "Did you find some tunes more difficult than others? If so, why do you think it was so?"

19 claimed still to hear the tunes, but 16 of these had had to sing them 'to themselves';
18 could hear only vague sections;
the 3 remaining subjects, for whom the whole had faded, included the 2 who had originally claimed to have had 'clear images.' Only 7 stated that no tune had been found more difficult than the others.

It seems reasonable to suggest, in passing, that any difficulty in the recall of an auditory experience which is due to the inter-relationship of the elements of the experience negatives the assumption that the recall is an auditory image at all, for a genuine auditory image, being the recurrence of a perceptual experience, would occur or not occur regardless of the inter-relation of its constituent elements.

The suggested reasons for the relative difficulty experienced in recalling some of the tunes are interesting. Some found the second and fourth tunes more difficult than the other tunes because they contained notes of unequal duration. Reference was made to the "unexpected nature of the fourth tune," "the tune which jumped about." 'Expectancy,' in this connection, can only mean that certain sound relations have been experienced by the subject, in conventional association, with sufficient frequency to have established a disposition for the occurrence of one to recall the habitually associated relations or correlates. This suggests further evidence of the importance of habit formation in the processes of recall, and of the relative futility of reliance on the perceptual recurrence of a new auditory experience. Several references were made to the *length* of the examples, as constituting a factor of difficulty in recall, although in this case the longest example contained only eight notes in melodic sequence.

The introspections following the playing of the short non-rhythmic tunes, in the supplementary experiments carried out in the Birmingham University Psychological Laboratory, confirm those of the students' and children's groups. Typical introspections were:

F. M. A.: "Something of a unity remained for some time, then faded completely, and I could get no auditory image when I tried. An attempt brought a visual image of the piano."

"Unable to get any recall beyond a vague pattern. This has now faded completely."

J. M.: "I could hear the last note as an image, or rather as a perseveration of the experience. I can sing the whole tune to myself, but it is unrecallable as an image, in the sense of a recurrence of the auditory experience. I recall the tune as something I have sung."

F. E. M.: "Last note remained for a short time. Then the tune was vaguely recalled. I found myself singing it to myself."

"I have a decided motor 'image' of my fingers moving."

The recall of harmony.

The introspective analysis of the processes involved in recalling chords, *i.e.* sounds of different pitch heard simultaneously in harmonic relation, demands experience both in the recall of such sound structures and in the effort to analyse the process of such recall. As neither is a common experience, it is difficult to obtain much reliable information on this kind of recall.

If the recalled harmony is merely the recurrence of a perceptual experience, the recall of a chord heard, *i.e.* an auditory image of a chord, the notes would be heard together as a chord, as they were presented.

If the experiencer, however, has acquired the ability to reproduce on some instrument, such as a piano or organ, the chord he has heard, the tendency to utilize this associated kinaesthetic experience, in order to think in terms of harmony, would be unavoidable, and such a subject might recall harmonies either as chords or as arpeggios.

When a subject has no instrumental experience and has no kinaesthetic associations other than vocal ones, any harmony recall which is not purely auditory must be in the nature of an arpeggio, *i.e.* a series of sounds, as the voice cannot produce a chord.

In thinking of a series of chords, my own general tendency appears from introspection to be to subvocalize the highest notes of the series and to experience those kinaesthetic manual impulses which would be involved in playing the actual harmonies, while the 'root' series frequently, but not always, tends to occur as a series of 'pedal' impulses. While the whole process is essentially kinaesthetic, I am aware of the sounds which would occur if the motor impulses experienced were allowed to materialize as stimuli of the appropriate mechanism.

The following introspective analysis by Dr Austin suggests a similar conclusion: "I recall the chord as a chord without sound, or rather without concrete sound; any auditory image is very vague, if it exists at all; I employ motor imagery to build it up."

Although the introspections of the students in reply to the questions on harmony recall show a not unnatural hesitancy and uncertainty, they definitely tend to emphasize the reliance on acquired motor associations rather than on auditory imagery.

Twenty-nine of the forty claimed to have recalled the harmony as a chord and not as an arpeggio, but twenty-five of these recalled either the highest or lowest note more clearly than the other notes. This can only mean that the recall, inasmuch as it differed from the original experience, was not a purely perceptual recall, *i.e.* not a simple auditory image, and the special prominence of either the top or bottom note must have been due to kinaesthetic reinforcement, probably vocal.

Only ten of the forty disclaimed the use of motor help in the recall, and it was not unnatural that these should have claimed to recall the experience as a chord and not as an arpeggio.

General conclusions suggested by the introspections.

From the introspections both of the children and of the adults, there can be no reasonable doubt that some form of associated kinaesthetic recurrence, either vocal or manual, or even pedal, is an essential factor in the adequate recall of auditory musical experience. Any purely perceptual recall, *i.e.* an unaided auditory image, is vague, fleeting, fragmentary; limited to the shortest of melodic passages, or to the mere perseveration of the last few notes. The experiments suggest that the general tendency is to translate the fleeting auditory recall immediately into those appropriate kinaesthetic impulses which previous experience has inseparably connected with the associated sounds. It is deducible that not only does the sound tend to stimulate the appropriate motor association, but that the process of thinking in terms of sound is essentially a process of 'thinking' in recurred kinaesthetic experience.

The results of the three tests employed in the children's experiment, the recall test, the imagery test and the intelligence test, were subjected to statistical analysis. The method adopted and the results are discussed in Section V.

V. STATISTICAL ANALYSIS.

Method and reliability of analysis. A difficulty in the accurate evaluation of the results of the kind of auditory recall test employed in this investigation is that the measurement of the ability to *recall* includes a measure of the ability to *educe* the various relations of pitch and rhythm on which the questions were based. To eliminate this factor, the following procedure was adopted. A school was chosen for the experiments where skilled and interested attention was given to musical training. A selected group was then tested in rhythm and pitch differentiation, the tests employed being those published in the *British Journal of Educational Psychology*, vol. I, pp. 313–21, Test Series A and B. The standard of achievement was exceptionally high, the mean score of the pooled tests being 86·2 per cent. On the first submission of the tests the mean score for pitch differentiation was 89·4 per cent., and as high as 90·4 per cent. when the tests were resubmitted. Only those testees who obtained at least 80·0 per cent. in the pooled tests were admitted to the subsequent tests on recall.

The tests in eduction and the recall tests were submitted twice, and provided respectively reliability coefficients of 0·84 ± 0·026 and 0·76 ± 0·040. The Imagery Tests were evaluated by awarding a score of 2 for a 'clear' image, 1 for a 'vague' image, and 0 for 'no image.'

The correlation coefficients were calculated by the product-moment formula; age was partialled out.

Table of intercorrelations.

	Auditory recall	Imagery tests			
		Auditory	Visual	Motor	*g*
Auditory recall	—	0·00 ± ·096	– 0·14 ± 0·088	0·18 ± 0·088	0·53 ± 0·064
Auditory imagery	0·00	—	0·37 ± 0·077	0·38 ± 0·077	– 0·14 ± 0·088
Visual imagery	– 0·14	0·37	—	0·20 ± 0·085	0·03 ± 0·089
Motor imagery	0·18	0·38	0·20	—	– 0·14 ± 0·088
g	0·53	– 0·14	– 0·03	– 0·14	—

The scores of the Imagery Tests, while showing positive and significant intercorrelations, exhibit no such tendencies with anything else, but decided tendencies to negative or insignificant correlations with '*g*,' a result which corroborates the findings of Miss Carey (9), Dr Aveling (10) and others.

The results of Whipple's (11) and other investigations are corroborated by the lack of significant correlation between the analytical recall tests and the imagery tests[1].

One of the most striking coefficients in the above table is that indicating the high degree of correlation shown in this experiment between the scores gained in the recall tests and the '*g*' tests. In the hope of eliminating the factor of *ability to educe* from the measurement of the *ability to recall*, only those testees who obtained over 80 per cent. in the pooled perception tests were admitted to the recall tests. If, however, the suggestion already made is true, that the accuracy and content of recalled experience depend mainly on the accuracy and content of the actual experience, the procedure adopted would only have the effect of raising the standard of the scores gained in the recall tests, and of making these scores more closely approximate to those which would have been gained had the tests been submitted as mere perceptual or eduction tests. In other words, the greater the perceptual efficiency, the less is the measurement of the accuracy of recalled experience likely to be a measure of anything other than the ability to educe the relevant relations. The truth of this is further evidenced by the unexpectedly

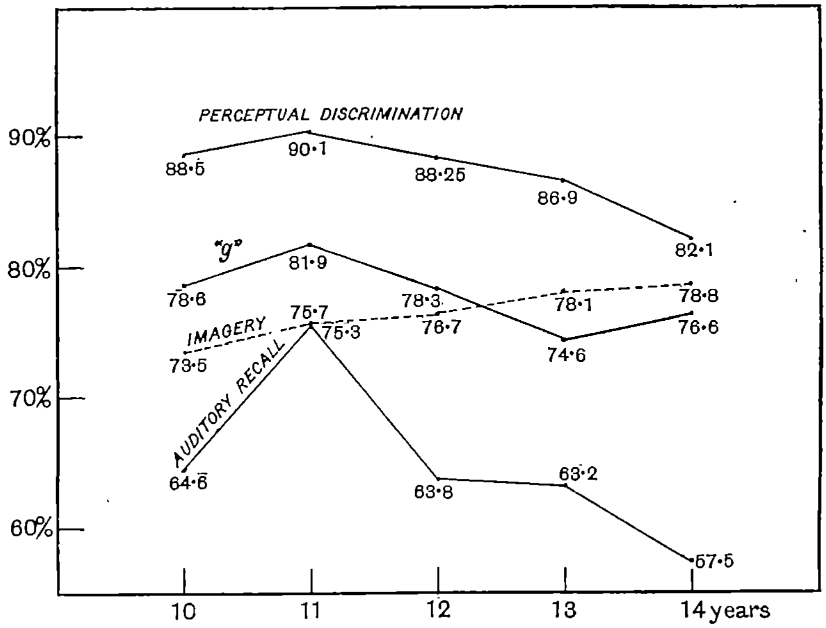

Fig. 1. Figure showing independence of age-imagery curve.

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[1] See also (12).

high correlation of 0·71 ± 0·047 found between the scores of the recall tests and the perceptual discrimination tests, and by the lack of positive correlation between the recall and the imagery tests.

Curves showing the mean scores in each of the tests gained by each age group were plotted. While they have no value as indications of the influence of age on the development of the abilities owing to the selection of the testees, they very definitely corroborate the previous evidence as to the parallelism of the curves, as suggested by a previous investigation made by the writer (8), and illustrate the independence and consistent rise of the imagery curve.

VI. SUMMARY OF RESULTS AND CONCLUSIONS.

1. The results of the investigation suggest that auditory imagery is a relatively negligible element in the recall of musical experience. It is fleeting and elusive, fragmentary and vague, and limited in content to the attributes and relations cognized at the time of the original experience.

2. A far more important aid to music recall seems indisputably to be some form of kinaesthetic recurrence. This may function in two ways: either as a mechanized motor habit sequence, or as the immediate recurrence of those kinaesthetic impulses which have become associated to given auditory experience.

3. The 'memorization' of music, for the purpose of instrumental or vocal reproduction, seems mainly to consist in the formation of a motor habit sequence, and any reliance on auditory experience alone seems to be so restricted as to be negligible in practice.

4. The results of the experiments further suggest that purely perceptual auditory recall is equally inadequate as a factor in the process of 'thinking in music,' especially in the recall of harmony, and suggest that the 'thinking' tends generally to include the recurrence of associated kinaesthetic experience, and frequently seems to consist almost entirely in the recurrence of such motor associations.

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(12) Bentley, I. M. (1899). *Ibid.* ii, 1.

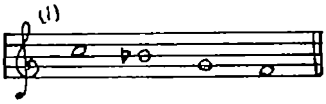
APPENDIX

Test Series D. Extended tests on recall of musical experience.

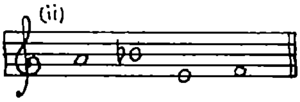
Test 1. *Melodic recall.* (Submitted to Children's Group.)

It is essential that the subjects do not know what questions they are going to be asked, when the tune is being played. Allow ten seconds after each tune before asking the questions.

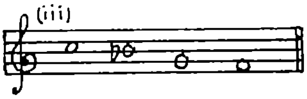
1. Tester: I am going to play three little tunes. Listen:



Now think over that tune in your mind. (Allow ten seconds.) Now listen again:



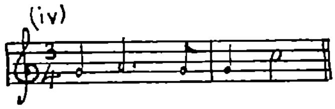
Once more:



Was the last tune I played the same as the first or second tune I played, or was it a different tune altogether?

2. How many notes were there in the last tune I played? (It must not be played again, even if no one can answer the question.)

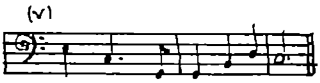
3. Listen again:



How many notes were there in that tune?

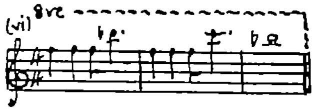
4. Did the first two notes go up, or down, or were they the same?

5. Listen again:



What kind of beating would suit that tune best, 2, 3, or 4?

6. Listen again:



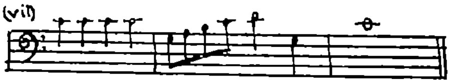
How many notes were the same at the beginning?

7. How many notes were there altogether?

8. Was it in 2, 3, or 4 beats?

9. Did the last two notes go up, or down, or were they the same?

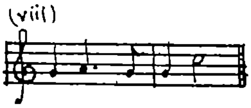
10. (For this example use a violin.) Listen again:



Did the last two notes go up, or down, or were they the same?

11. How many notes at the beginning were the same?

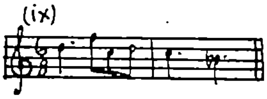
12. (Again on the violin.) Listen again:



Have I played that tune before?

13. How many notes were there in it?

14. (For this use a flute, or fife or whistle.) Listen again:

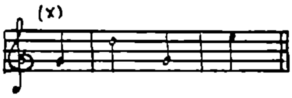


Did the first two notes go up, or down, or were they the same?

15. Have I played that tune before?

16. (Play same tune again on the piano.) If I have played that tune before, on what instrument did I play it?

17. (Sing the following example.)



Which was the highest note, the first, second, third or fourth?

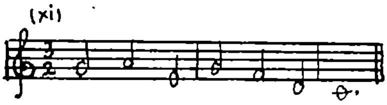
18. Did the last two notes go up, or down, or were they the same?

19. (Sing the example played in No. 1.) How many notes were there in that example?

20. If I have played it before, on what instrument did I do so?

Test 2. *Modified test as submitted to adults' group.*

PIANO:



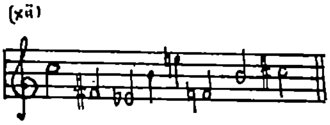
1. Recall the tune. Did the first two notes go up, or down, or were they the same note repeated?

2. How many notes descended at the end?

3. Are the first and last notes the same? If not, which is the higher?

4. Was the passage in duple or triple time?

PIANO:



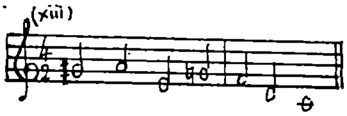
5. How many notes were there in that passage?

6. How many notes descended at the beginning?

7. Which was the highest note?

8. What relation existed between the first and last notes? Say simply whether they were the same, or which was the higher.

VOICE:

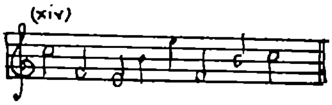


9. Enumerate any differences you can recall between that tune and the one I played first.

10. How many notes were there?

11. Did any note occur more than once? If so, which?

VOICE:



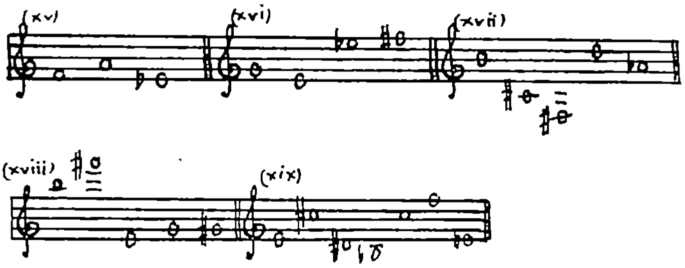
12. Enumerate any differences you can recall between that tune and the second I played.

13. How many notes were there?

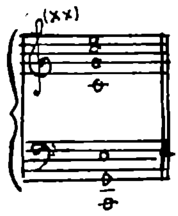
14. Did any note occur more than once? If so, which notes were the same?

The introspective questions are included in the text.

Test 3. '*Nonsense*' *tunes used in the supplementary test.*



Test 4. *Harmonic recall test.*



1. How many notes were there in that chord?

2. Can you hear them now?

3. Do you hear the notes, if at all, as a chord, or do you hear them separately, as an arpeggio?

4. Can you still hear the example I played last?

5. Does any note stand out more clearly than the others? If so, which?

6. Do you employ or experience any form of motor 'image,' either vocal or manual?

7. Can you hear in your mind two or more notes exactly together?

8. If you think you can, is one more prominent than the other? Or do you hear them equally clearly and with equal intensity? If not, which is the clearer, the higher or the lower?

9. Can you hear a chord in your mind without motor assistance?

TEST SERIES E. NON-MUSICAL IMAGERY AND RECALL TESTS.

Test 1. *Auditory imagery.*

1. Think of the sound made by a dog barking. Can you hear it in your mind, clearly, vaguely, or not at all?

2. Did you think of any particular dog or not?

3. Could you see the dog in your mind when you heard it bark?

4. Was it a loud, savage bark, or a little yap, or how could you describe it?

Say whether you can hear in your mind clearly, vaguely, or not at all the following sounds:

5. The sound made by a horse when running along a hard road.

6. The sound made by a wet cloth flung on to a floor.

7. The sound made by a ruler drawn along railings or a radiator.

8. The sound made by sawing wood.

9. The sound made by thunder.
10. The sound made by a clock striking.
11. Was it a particular clock you heard, or was it just any clock?
12. Did it chime or just strike?

Test 2. *Visual imagery.*

Where possible, answer 'clearly,' 'vaguely,' or 'not at all.'

13. Can you see in your 'mind's eye' a man's silk hat?
14. Was it much taller than it was wide?
15. Can you see a tree?
16. Was it a big tree or a little tree?
17. Were there leaves on it?
18. Did you see it as a kind of shape, or did you see it coloured?
19. Can you see a pendulum swinging?
20. Can you see a colour in your mind?
21. What colour did you see?
22. Was it a coloured thing or just a colour?
23. Can you see in your mind the kind of reddish colour called brick-colour?
24. Did you actually think of a coloured brick, or just the colour apart from a brick?
25. Can you see in your mind a daffodil?
26. Was it growing, or in a vase or bowl, or just a daffodil?
27. Which way was the trumpet pointing, to the right or to the left, or straight up?

Test 3. *Motor 'imagery.'*

28. In the same way can you feel what it is like to be running upstairs? Say whether you can imagine the movements clearly, vaguely, or not at all.

Say whether you can feel clearly, vaguely, or not at all the movements you would make in doing the following things:

29. Kicking a ball.
30. Sharpening a pencil.
31. Throwing something.
32. Lifting a heavy weight.
33. Putting on your clothes.
34. Using a knife and fork.
35. Washing your hands.
36. Breaking a stick.
37. Drying yourself after a bath.
38. Pushing someone.

KEY TO TESTS.

Series D. Test 1.

1. Same as first.
2. 4.
3. 5.
4. Up.
5. 3.
6. 3.
7. 9.
8. 4.
9. Down.
10. Up.
11. 4.
12. Yes.
13. 5.
14. Up.
15. No.
16. Piano.
17. 4th.
18. Up.
19. 4.
20. Piano.

Series D. Test 2.

1. Up.
2. 4.
3. 1st.
4. Triple.
5. 8.
6. 3.
7. 5th.
8. Last was the higher.
9. 1st triple, this duple or quadruple.
   1st note in this raised a semitone.
10. 7.
11. No.
12. This tune is a diatonic variation of the previous tune.
13. 8.
14. 1st and last.
    2nd and 5th.
    4th and 7th.

(*Manuscript received* 1 *July*, 1932.)

TEMPERAMENT TESTS. I. TEMPERAMENT.

BY RAYMOND B. CATTELL.

I. *Some obstacles in the psychology of temperament* (pp. 308–9).
II. *On the method of estimating personality traits* (pp. 309–11).
III. *Trait estimates in the present research* (pp. 311–17).
IV. *Analysis of trait intercorrelations* (pp. 317–25).
V. *Discussion and summary: the Surgent temperament* (pp. 325–8).
References (pp. 328–9).

I. SOME OBSTACLES IN THE PSYCHOLOGY OF TEMPERAMENT.

AT the present time the study of temperament is one of the most unsatisfactory, as far as real progress is concerned, in the whole field of psychology. This state of affairs is partly due to the difficulty in approaching an aspect of mind originally arbitrarily defined as that part of mental life determined by physiological, particularly metabolic, factors. It is no less due to the unconscionable excess of speculative over experimental treatment.

There is an intrinsic persuasiveness in an artistically elaborated type picture, which few minds can resist. Once a new type distinction has been advertised and discussed, it continues, however faulty it may be from an objective standpoint, to distort reality to its own image and to acquire convincingness with constant use.

Many of the temperament types which have been independently launched are nevertheless extremely similar. This convergence of conceptions justifies the surmise that some true objective distinction is approached by them. Everyone must have been struck, for example, by the similarity of the introvert-extravert distinction of Jung (16) and McDougall (18) to the schizothyme-cyclothyme polarity sponsored by Bleuler (4) and Kretschmer (17). The introvert, again, has considerable resemblance to the typical inferiority complex picture as drawn by Adler (1) and Wexberg (24). Both of these are barely distinguishable from the anal erotic character described by Freud (7) and Jones (14), though these writers, like Adler, are not disposed to think of different mentalities and behaviour patterns in terms of temperament[1].

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[1] As far as I know, not one of these authors has acknowledged any affinity of his type conception to other conceptions—with the exception of McDougall, who devotes a penetrating discussion to the points of distinction which may exist between introvert and schizothyme (18), p. 392).

Behind the introvert-extravert distinctions lies a succession of similarly orientated type distinctions, passing through the reflective and active types of Jordan (15), the tender and tough-minded types of James (13), the unstable and stable types of Trotter (22), the subjective and objective types of Stern (21) and, on a more valuable experimental basis, the types described by Jaensch (12) and by Heymans and Wiersma (11). Away from this line of development, of which only a few illustrations are selected here, there are innumerable other type distinctions, more or less on a basis of temperament, which are regarded as fundamental by their authors; *e.g.* the gland types of Berman (3), racial types (9), the sadistic-masochistic, male and female, types of Apfelbach (2), the 'economic,' 'aesthetic' and 'religious' types of Spranger (20).

The present research is an attempt to determine the principal patterns of temperament and character which actually exist, using carefully controlled estimates, particularly of those traits which appear in the introvert-extravert distinction and in those distinctions of which it may be said to be the prototype.

II. ON THE METHOD OF ESTIMATING PERSONALITY TRAITS.

A preliminary list was made of the characteristics (in pairs of opposites) which are usually regarded[1] as involved in each of the following four types: introvert, schizothyme, inferiority complex and anal erotic. This resulted in twenty-eight traits (actually twenty-eight pairs of opposites expressing introvert-extravert distinctions, etc.) of which seventeen belonged to the introvert, nineteen to the schizoid, seventeen to the inferiority complex and twenty to the anal erotic. In every case the overlap was very great and indeed practically complete for the nuclear and most significant traits in each type[2].

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[1] The authorities consulted for the full description of each type were not the original authors of the distinctions but later interpreters who propound more developed conceptions which have gained in clarity from years of discussion and criticism. Thus introversion-extraversion was taken not from Jung (16) (where it is, moreover, confused by cross classifications) but from McDougall, *An Outline of Abnormal Psychology*, p. 435 (18); *National Welfare and National Decay*, p. 98. Schizothyme-cyclothyme distinctions were taken from Kretschmer, *Physique and Character*, pp. 115–53 (17). Similarly, the inferiority complex pattern was taken, not from Adler, but from the developed views of his disciple Wexberg, *Individual Psychology*, 1930 (24) and the anal erotic character from Ernest Jones, *Papers on Psycho-analysis* (14).

[2] The detailed analysis is given in a thesis, "Temperament tests and perseveration," by the present author, in the University of London Library (5). Much of the supporting data throughout this article has been left in the thesis referred to.

In all researches involving trait estimates one is faced with the difficulty that many of the traits given for estimation may not exist as unitary characters. Much criticism has been directed against estimates on this and other counts, but a good deal of it is beside the point, especially in view of the limited objectives usually set at this stage of temperament and character research. To say, for example, that no such trait as timidity can exist, since one and the same child may be timid before his teacher, bold before his class-mates, unafraid of physical dangers and yet cringing before social disapproval, is to overlook the possibility that a general tendency to timidity may exist, greater for some children than for others, but variously conditioned in different children to particular situations. Yet although this diversity of reaction in differing situations does not always disprove the existence of a unitary tendency, it may sometimes do so. Being ignorant of the real state of affairs, we can only proceed safely by splitting up into finer and finer traits the broad traits upon which earlier estimates were somewhat uncritically made. If the correlations between the subdivisions approach unity, then it will be possible to return to the original broad trait, but, if not, the supposed unitary traits must be again split up until elemental behaviour patterns are found. Almost all the traits in the above composite temperament picture were split up into more restricted forms of behaviour, the particular divisions adopted being determined by observation and the indications of earlier work, *e.g.* Webb's (23) 'Sociability,' which occurred in three of the four type distinctions mentioned, was split up into three traits: (1) liking to meet people as opposed to avoiding contacts, (2) fondness for large groups and crowds as opposed to small exclusive groups of two or three, (3) reacting readily and sympathetically to the emotions of others as opposed to lameness and awkwardness of response.

Other sources of error in estimates were obviated as far as possible in the following ways. (1) Judges were specifically warned and trained to eliminate any attitude of approval or disapproval, especially moral, of traits or of individuals. By this we hoped to counteract the well-known 'aura' effect. Further, all traits were not estimated for one individual at once, neither were the estimates made as numerical calculations, but each trait was taken separately and the subjects were simply ranked in order of their possession of that quality. (2) Errors due to personal prejudices of the judges were counteracted by increasing the number of judges, in this instance to four. (3) Errors due to subjects being seen from only one aspect or in a limited field, *e.g.* in judgments by superiors, subordinates, parents or teachers, or only at work or at play, were guarded against by the selection of fellow-students as judges. All estimates were given by judges specially chosen on the grounds of their wide knowledge and constant association with the subjects in study, rest and recreation in a residential college. To get rid of errors due to estimating from impression and intuition instead of from actual behaviour, the judges were instructed to keep observation on their subjects for two terms and to compile records of actual incidents, from a study of which the rankings were eventually made. (4) Errors due to the judges having preconceived ideas of temperament patterns were as far as possible eliminated by the choice of educated, intelligent, scientifically minded persons as judges who were as yet unread in the psychology of temperament types. They were further warned not to imagine that traits occurring close together in the list were necessarily in any way related.

The subjects were sixty-two male (second- and third-year) students in a training college ranging from 18 to 25 years of age. They were rather older and less typical than university students, many of them having worked in industry between leaving school and coming to college.

III. TRAIT ESTIMATES IN THE PRESENT RESEARCH.

It was felt that the value of the enquiry as a whole would be enhanced by including some traits, other than the above temperament traits, among the estimated characteristics, in order to provide landmarks by which to orientate whatever general patterns might be discovered. Especially was it desirable to relate the patterns to Webb's (23) 'will' factor, and Garnett's (8) 'cleverness' factor, which gives many indications of being a temperamental tendency.

Accordingly the following additions were made to the composite temperament list:

(*a*) Traits most highly associated with Webb's '*w*' factor:

Perseverance—Wilful Changeability.
Persistence (in face of obstacles)—Changeability.
Conscientiousness—Unreliability.
Kindness on Principle—Absence of Kindness on Principle.
Interest in Religion—Absence of Interest in Religion.

The last of these had eventually to be dropped, as the judges announced themselves unable to make reliable estimates of it, external behaviour being no guide.

(*b*) A set of traits correlating most highly with the suspected '*c*' factor of Garnett:

General Cheerfulness.
Sense of Humour (already in temperament list).
Quickness of Apprehension.
Originality of Ideas.

(*c*) Some non-intellectual traits, a knowledge of which has been much in demand in vocational guidance work:

Tact.
Strength of Personality.

(*d*) A measurement of intelligence (Cattell Group Test, Scale III A (6)). These additions brought the number of traits to 48 (46, when Interest in Religion and Intelligence are omitted). The precise significance given to each verbal label is set out below. In every case the traits are given in pairs of opposites. In this way the quality is more precisely defined than if only the positive trait were described: the judge becomes accustomed to assessing each subject as it were by his position on a line, both ends of which are fixed.

SOCIABLE Mixes well. Likes to meet people. Makes contact with strangers easily and unreluctantly.	UNSOCIABLE Not fond of human intercourse for its own sake. Avoids meeting people, especially strangers. Dislikes human contacts.
GREGARIOUS Likes to be in crowds. Fond of being in human society. Usually has a large circle of acquaintances.	EXCLUSIVE Fond of solitude. Spends much time alone. Has one friend or small exclusive group.
RESPONSIVE Affected easily by emotion of others. Responds easily and extensively. Reacts warmly, sympathetically, to one's emotional moods.	UNRESPONSIVE Deficient, awkward, lame or repressed response to joy, laughter, sorrow, anger, etc., in others.
CO-OPERATIVE Open-hearted, treats others fairly, ready to co-operate and make the first move of approach.	DISTRUSTFUL Mixture of fear and hostility in attitude to others. Distrustful of co-operative advances. Non-co-operative (for this reason only).
GOOD-NATURED Generous. Well-wishing. Enjoys other's success. Good-natured in sense of being more ready to give than to take in small things. Not necessarily prepared for great or heroic self-sacrifice and unselfishness.	MALICIOUS Cruel. Laughter sardonic. Disparages others. Inclined to be interested in and to enjoy other's misfortunes.

HUMOROUS Turns readily and passes easily to the humorous aspect of things. Fond of witty sallies and joking. Takes few things seriously.	EARNEST Inclined to be humourless. Deadly earnest always. Not deflected easily into humour. Seldom makes jokes spontaneously—in passing.
BALANCED Jolly. Happy with simple pleasures. Satisfied with life's compromises. Finds his pleasure in mundane, everyday things. Well-balanced and integrated emotional attitudes.	EXTREME Inclined to the tragic and heroic outlook. Experiences both profound pathos and ecstasy of life. Goes to extremes. Things must be just perfect to be enjoyed. All or nothing.
HASTY Simple, direct. Not given to analysing motives. Acts without great deliberation.	INTROSPECTIVE Turned inwards. Introspective. Given to brooding reflection. Hesitating. Preoccupied with self-analysis, analysis of his own thoughts and motives.
PLACID Does not spend much time in mental conflict. Not liable to intense inner struggles, and gets over them quickly and easily. Placid. Mentally comfortable and well-integrated.	GIVEN TO MENTAL CONFLICT Subject to prolonged mental conflicts (*e.g.* to self-reproach, convictions of sin, as in religious 'conversion'). Worries over mental state. Manner sometimes strained and eccentric.
CONFIDENT Confident. Solid without pretence or show of power. Takes up life's responsibilities naturally, without hesitation.	ANXIOUS Fear of, and difficulty in, making decisions. Procrastinative. Afraid of responsibility. Seeks positions of safety and security, flees from risks and trys to avoid life's responsibilities. Nerve fails at last moment.
OPTIMISTIC Inclined to believe that all is going well. Sees external world in bright colours. No fear for the future. Not necessarily highly cheerful in manner, but possessed of a general unobtrusive optimism. The worst possibilities seldom occur to him.	PESSIMISTIC Shows fundamental discouragement in regard to life. Inclined to be pessimistic in all things. Sees worst possibilities and is inclined to believe in approaching catastrophies—'things going to the dogs.'
GENERAL CHEERFULNESS Inclined on the whole to be merry, gay, cheerful, for long periods. Indulges in light-hearted laughter.	GENERAL GLOOMINESS Tendency to be gloomy and sad, or at least 'stodgy' and bored. Not melting easily into happy laughter.
ORTHODOX Shows no desire to stray from accepted beliefs and behaviour. Amenable to demands of group and does things 'because they are done.' Does naturally what people expect of him.	INDEPENDENT Tends to espouse eccentric ideas. Given to independence and independent thinking. Sticks to his own ideas if he arrives at original conclusions. Sometimes a rebel against authority.
AMENABLE Loses spirit and energy if faced with overwhelming difficulties and opposition of society. Gives in gracefully (without vindictiveness).	OBSTINATE Obstinate, stubborn, does not give in when faced with considerable opposition. Opinionated. Does not conform or change without morose ill-will.

OBJECTIVE Accepts readily the logic of facts. Not helped or hindered by theories. Realistic in sense of accepting facts as they are, without subjective colouring.	SUBJECTIVE Prone to start with theories and attempt to fix facts to them. Inclined to feel own ideas and beliefs are true. Dogmatic. Subject to delusions.
NAÏVE Ignorant of self. Simple and direct in motives. Can be blissfully unaware of real sources (good or bad) of his beliefs and actions.	SOPHISTICATED Sophisticated, tortuous, subtle. Complicated in motives. Rarely does anything from a single motive (not dishonest), and is well aware of ultimate origin of his motives. Does not deceive himself.
FRANK Open, frank, willing to talk about his affairs and plans. Expresses his real thoughts and feelings.	SECRETIVE Secretive, hides his thoughts, feelings and intentions. Remains a riddle to acquaintances. Keeps his plans to himself and sometimes deceives without any apparent object.
FORWARD Tendency to self-display, showing off, boasting, love of adornment. Not afraid of making a fool of himself. Confident in company, unembarrassed.	SHY Self-conscious, embarrassed in company, diffident, retiring, shy, shrinks from self-display.
NATURAL Naturally graceful and charming, makes easy contacts without formality. Is natural and easy with all kinds of people.	FORMAL Prefers human contacts to be regulated by etiquette and cold formality. Is always precise and conventional in greeting, etc. Impersonal.
VERBOSE Talkative and expressive, apparently rich in ideas. Talks a good deal even about unimportant matters.	TACITURN Quiet. Reserved, not much inclined to talk. Only expresses himself when absolutely necessary.
CONTENTED Enjoys life as it comes. Ambitions extend only to immediate, practical goals. Shows no desire to achieve anything great.	AMBITIOUS Preoccupied with what he will make of himself. Concerned with distant goals of achievement and dreams of individual development. Highly ambitious, striving for power.
FRIENDLY Treats all classes and individuals equally. Does not constantly aim at distinction as such. No display of coldness, haughtiness, or offensive superiority.	PROUD Proud. Snobbish (veiled or open). Desires to be thought a bit better than others. Believes himself to be of slightly better quality.
ENERGETIC Free flow of energy. Naturally industrious. Naturally willing to work.	INERT Poor flow of energy. Apparently half-hearted. Lame, lazy behaviour. Tendency to avoid work, malingering, illness as excuse.
CONSCIENTIOUS Essentially possessed of a strong conscience. Trustworthy and reliable where morals enter into question. Strong sense of moral duty and of right and wrong.	UNRELIABLE Able to hoodwink his conscience. Not entirely to be trusted in moral matters and in temptation.

CASUAL	Inclined to be casual and perhaps negligent of minor matters. Does not insist on law being obeyed to the last letter, even by others.	PEDANTIC	Pedantic, fussy, too conscientious over trifles. A bit of a martinet with others.
EMOTIONAL	Subject frequently and intensely to strong emotions, anger, pity, joy, fear, love, good fellowship, grief, etc.	UNEMOTIONAL	Not merely weak on the surface in emotion but also in deeper layers. Always calm and guided by reason.
EXPRESSIVE	Emotion demands ready expression. Results readily and easily in words, actions, gestures. Impulsive. Excitable.	INHIBITED	Emotion bottled up and hidden. Emotion evident by silence rather than word or deed. Action long deferred.
ADAPTABLE	Adaptable to changes in circumstances. Not upset by enforced changes of home, work, habits. Likes to have changes.	CONSERVATIVE	Upset by anything demanding change in habit. Conservative in small things and retains accustomed things (not necessarily in politics). Likes same old walks, tunes, neighbourhood, books.
NOT HEDONISTIC	Indifferent to minor sensuous pleasures. Not seeming to need them (chocolates, smoking). Even ascetic and self-denying where pleasures are concerned.	SENSUOUS	Constant craving for sensuous and sensual pleasures (not craving for excitement or comfort merely). Seeking solace in eating, smoking.
CARELESS	No strong tendency to collect or acquire. Not primarily and fundamentally interested in financial matters.	AVARICIOUS	Strong tendency to collect and acquire. Greedy of gain. Stores up. Is interested in all money matters.
GENEROUS	Generous, careless in use of money. No sense of value of things. Easy and broad charity.	STINGY	Chary of spending. Not giving unless there is a good cause, or on principle. Mean. Always complains of overcharge.
GRACEFUL	Bodily movements well integrated and graceful. Not obtrusive. Easy and natural gesture.	AWKWARD	Bodily movements jerky and uncontrolled. Awkward and 'gawky' in gait and movement. Clumsy gesture.
INDIFFERENT	Takes hard knocks without fuss. Not so susceptible to hard words or upset by emotional disagreements of others. Not so reactive to praise and blame of others. Thick skinned.	HYPERSENSITIVE	Easily upset by emotional disagreement. Easily wounded by tactless remarks. Highly sensitive, especially where self-esteem is concerned. Responds to slightest hint.
MATERIALISTIC	Much affected by material conditions and circumstances. Loves comfort, material possessions, convenience, neatness. Trifles matter for him a good deal. Not inclined to ignore and put up with unfavourable living conditions.	DETACHED	Neglects and is indifferent to material circumstances. Absent-minded and uninterested where food, dress, tidiness, etc., are concerned. Capable of stoical indifference to comfort and amenities.

MATURE Ready always to make a compromise between own wishes and those of others. Able to forego without fuss. Does not overlook subjective life of others, their wishes, limitations and individualities.	WILFUL Wilful, not able to swerve from his own desires and goals. Demands (instinctive) satisfactions and upsets everybody if he doesn't get them. Childish unwillingness to adapt and respond to external world.
REALISTIC Lives very much in real, active world. Alert (awake to objects) and practical (not necessarily efficient, skilled or practical). Sees things just as they are and reacts as obvious necessities of situation demand.	AUTISTIC Builds up his own world of ideas. Day-dreams, reads excessively. Attached to ideal systems. Idealizes remote things. 'Imaginative' in some senses. Sees things distorted by his own beliefs.
MATTER-OF-FACT Enjoys and accepts variety and individuality of real world. Reacts to each situation as a new thing, without fitting it into a preconceived mould. Is led by common-sense and his own feelings. Unable to think out things on principles.	PRINCIPLED (SYSTEMATIZING) Can relate any of his actions and beliefs to a principle. Systematizes everything. Is more interested in generalizations and abstractions than things and examples. Deals with laws and types.
EMOTIONAL MOOD OSCILLATING Subject to fairly periodic oscillations of mood between gaiety and depression. Affected powerfully by general moods, being depressed or excited and lively for days at a time.	STEADY Shows a fairly even and constant emotional mood. Not deflected for days at a time by phases. Even flow of energy and even temper.
CHANGEABLE Takes up things and drops them again because of changes of interest. Changeable by nature.	PERSISTENT Activities and purposes naturally sustained. Constant (not merely natural vigour).
PERSEVERANCE Consistency of action through direction of will. Has persistent and unchanging motives, aims, goals.	WILFUL CHANGEABILITY Cannot be depended upon to persist in what he seems to have made up his mind to do.
KINDNESS ON PRINCIPLE Is consistently kind, not on impulse or in form of occasional and excessive generosity, but constantly and in manifold ways as a part of his beliefs.	ABSENCE OF KINDNESS ON PRINCIPLE Absence of that general benevolence for others which is based on principle and belief in being merciful and kind.
QUICKNESS OF APPREHENSION Sees point of anything at once. Quickly new material and takes in things at a glance. Jumps at conclusion and gives opinion accordingly.	SLOWNESS OF APPREHENSION Opposite qualities. Slow in thinking and in gathering situation. Heavy. Takes a long time to come to conclusion.
ORIGINALITY OF IDEAS Has many happy thoughts. Original ideas but not necessarily good or profound ones. New ideas, strange fancies, novel aspects of things occur to him.	BANALITY Opposite qualities.

TACTFUL	TACTLESS
Not necessarily gifted with ability to mix easily and get on with people, but always diplomatic, attentive to others' feeling reactions and able to make people comfortable in conversation.	Makes inopportune remarks (not merely stupid). Offends people unintentionally. Does not make others happy in conversation. Ignores possibilities and others' view-point.
IRRITABLE	GOOD TEMPERED
Given to constant irritability and sudden outbursts of anger.	Can always be approached without fear of irritability. Not irrationally angry.
STRENGTH OF PERSONALITY	LACK OF PERSONALITY
Extent to which person without obvious assertion impresses himself on the mind of a stranger, or on a group with which he is working. Not forgotten after an interview. Giving a good, pleasant and definite impression in conversation.	Uninteresting. Not noticed in a group. Leaving no impression. Lifeless.

IV. ANALYSIS OF TRAIT INTERCORRELATIONS.

Intercorrelation of estimates may be effected either by employing Spearman's 'foot rule' formula directly with the rankings as they stand, or by converting the subjects' rankings to quantitative values (assuming a normal distribution of the population in each trait) and applying the product-moment formula. Webb (23), finding practically no difference between the results obtained by the two methods, adopted the last named. For the sake of conformity we have done the same. In each trait ranking the subjects were divided into seven classes, corresponding to six equal increments of the quality concerned. The numbers in the seven successive classes giving the most nearly normal distribution in sixty-two subjects were 1, 6, 14, 20, 14, 6 and 1. The subjects in these classes were given respectively the quantitative valuations 7, 6, 5, 4, 3, 2 and 1.

The four judges—two second-year and two third-year students—first worked in complete independence but finally came together in pairs, the members of which discussed the rankings between themselves but remained independent of the other pair. Reliabilities were calculated between the rankings of these independent pairs.

After the 1104 correlations of traits and 48 reliability coefficients (which ranged from 0·12 to 0·72) had been worked out, a careful study of the correlation table[1] was made to discover the qualities which appeared to hang together in groups, with a view to arranging them in separate hierarchies. Three such groups of highly intercorrelated traits were eventually discovered.

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[1] Throughout this article a great deal of the supporting data usually set out in full has been omitted, since it can be consulted in the thesis already mentioned, which would otherwise require a monograph to communicate its content. The table referred to will be found on p. 46 of the thesis (5).

Table I. *The three groups of related traits first abstracted from the general correlation table.*

Group I.	Mean correlation with other traits in this group	Group II.	Mean correlation with other traits in this group	Group III.	Mean correlation with other traits in this group
(3) Cheerful	0·41	Conscientious	0·41	Balanced	0·36
(3) Natural	0·37	Persistent	0·40	(1) Cheerful	0·36
Sociable	0·37	Persevering	0·34	(1) Natural	0·34
Humorous	0·37	Energetic	0·30	(1) Responsive	0·31
Adaptable	0·35	(1) (3) Tactful	0·30	Frank	0·30
Gregarious	0·34	Emotional Mood Steady	0·29	(1) Optimistic	0·30
Quick of Apprehension	0·31	Mature	0·29	Generous	0·28
(2) Hasty	0·30	Pedantic	0·26	Objective	0·27
(3) Responsive	0·29	(1) Not Hedonistic	0·26	(1) (2) Realistic	0·25
Forward	0·29	(1) (3) Confident	0·26	(1) (2) Tactful	0·24
Verbose	0·29	Kindness on Principle	0·23	(1) (2) Confident	0·24
(3) Optimistic	0·25	(1) (3) Realistic	0·20	(1) Co-operative	0·23
(3) Co-operative	0·24	(1) Introspective	0·17	Amenable	0·20
Original	0·24	Good Natured	0·17	Matter of Fact	0·15
(2) Realistic	0·23			Strength of Personality	0·15
Expressive	0·22			Emotional	0·13
(2) Sensuous	0·21				
Placid	0·19				
(2) (3) Confident	0·17				
(2) (3) Tactful	0·15				
Sophisticated	0·10				
Contented	0·10				

In certain instances a trait in one group was also found to correlate highly with most of the traits in another group. Such instances have been indicated above by a bracketed number beside the trait, indicating the other group in which it will also be found. One perceives at once that the second of these groups has a strong resemblance to the group of traits correlating highly with '*w*' in Webb's results, whilst the first includes many of the traits which Garnett's '*c*' factor underlies (8). The third group, however, appears to be new.

Each of these supposed hierarchies was now examined by the application of the tetrad difference criterion to discover whether the conditions appropriate to the existence of a single general factor in each might be satisfied or whether the conditions indicated the presence of several group factors. The calculation of every tetrad in tables of this size would be a tremendous task and one not justified by the degree of accuracy normally obtaining in correlations based on estimates with populations of this number. Accordingly a sampling procedure was adopted, such that every coefficient was involved in at least three different tetrads and, in the case of the traits at the head of the hierarchy, in many more. Wherever the correlation of two traits produced suspiciously high tetrad differences (more than twice the P.E.) half a dozen more tetrads were calculated, especially those relating the suspect correlation to correlations already proved to be free of any connection other than that due to the general factor. Suspect correlations were also picked out by examining the rows and columns, as a check on the above procedure.

In this way the following correlations not satisfying the conditions of a hierarchy were isolated from groups 1, 2 and 3 respectively. The lists contain every correlation in association with which six or more tetrads had average tetrad differences of more than 1½ times the P.E.

Group 1.

	Tetrad difference greater than
Cheerful—Humorous	Approx. 4 P.E.
Introspective—Realistic	
Not Hedonistic—Tactful	Approx. 3 P.E.
Optimistic— „	
Responsive— „	
Verbose—Confident(–ive)	
Cheerful—Optimistic	
„ —Tactful	
„ —Responsive	
„ —Realistic	
Sophisticated—Content	
Humorous—Co-operative	
Verbose—Tactful(–ive)	
Confident—Tactful	
„ —Introspective	
Humorous—Original	
„ —Natural	
„ —Sophisticated	
„ —Responsive	
Humorous—Realistic	Approx. 2 P.E.
Cheerful—Adaptable	
„ —Not Hedonistic	
Confident— „ „ (–ive)	
„ —Realistic	
Introspective—Not Hedonistic	
Gregarious—Sociable	
Verbose—Expressive	
Cheerful—Gregarious	
Natural—Tactful	
Gregarious—Adaptable	Approx. 1½ P.E.
Humorous— „	

Group 2.

	Tetrad difference greater than
Kind on Principle—Good-Natured	Approx. 4 P.E.
Confident—Realistic	Approx. 3 P.E.
Not Hedonistic—Introspective	
Conscientious— ,,	
,, —Not Hedonistic	
Kind on Principle—Mature	
Persistent—Persevering	
Conscientious—Pedantic	
Introspective—Emotional Mood Steady	
Persistent—Conscientious	
Conscientious—Confident(– ive)	Approx. 2 P.E.
Persevering—Introspective	
Persistent—Good-Natured(– ive)	
Energetic—Not Hedonistic(– ive)	
,, —Good Natured(– ive)	
Good Natured—Mature	
Not Hedonistic—Realistic(– ive)	
Persevering—Mature	
Tactful—Kind on Principle	
Energetic—Emotional Mood Steady	
Mature—Tactful	
Confident—Introspective(– ive)	
Persistent—Emotional Mood Steady	
Kind on Principle—Realistic(– ive)	
Emotional Mood Steady—Persevering	Approx. 1½ P.E.
,, ,, ,, —Not Hedonistic	
Confident—Kind on Principle(– ive)	

Group 3.

	Tetrad difference greater than
Confident—Strength of Personality	Approx. 4 P.E.
Responsive—Matter-of-Fact(–ive)	
Confident—Realistic	
Strength of Personality—Responsive	
Tactful—Matter-of-Fact(– ive)	Approx. 3 P.E.
Strength of Personality—Co-operative	
Emotional—Realistic(– ive)	
,, —Generous	
Strength of Personality—Cheerful	
,, ,, —Realistic	
Responsive—Natural(– ive)	
Strength of Personality—Natural	
Frank—Emotional	
Responsive—Emotional	
Confident—Amenable(– ive)	
,, —Tactful(– ive)	
Generous—Realistic	

Group 3 (*cont.*)

	Tetrad difference greater than
Balanced—Emotional(– ive)	Approx. 2 P.E.
Objective— „ (– ive)	
Balanced—Amenable	
Responsive— „	
Generous—Co-operative	
„ —Confident(– ive)	
Balanced—Frank	
„ —Optimistic	
Emotional—Natural	
Cheerful—Optimistic	
Balanced—Confident	
Confident—Emotional(– ive)	
Frank—Matter-of-Fact	
Confident—Matter-of-Fact	Approx. 1½ P.E.

When these special linkages are arranged in diagrammatic form[1] so that the general patterns are more easily perceived, two groups (composed of traits linked together in practically all possible ways) stand out from the hierarchy of group 1, two from group 2, and, less clearly, two from group 3. In the last case no remainder in the form of a general factor is left over; the high correlations between many members of this group are due to the traits being saturated with both '*c*' and '*w*' (factors of group 1 and group 2).

Further examination showed, however, that one sub-group from group 1 was the same as one of the sub-groups from group 2—allowing for the absence of certain traits from the group 2 list. Similarly, a sub-group of group 3 was essentially the same as one from group 1.

Apart from the '*c*' and '*w*' hierarchies, therefore, four groups of specially related traits would appear to exist. The factors underlying the first two of these were contingently called 'c_1' and 'w_1', as they appeared associated with '*c*' and '*w*' traits respectively. When the various correlations of the 'c_1' and 'w_1' pools with the '*c*' and '*w*' pools themselves were worked out it was found, however, that the 'w_1' pool was largely composed of '*w*'-saturated traits which happened at the same time to be negatively related to '*c*'; whilst the 'c_1' pool was merely '*c*' traits which also possessed a good measure of '*w*.' Each set had stood out from the surrounding hierarchy, as if possessed of a group factor, simply because it belonged to the other hierarchy. It seems likely, nevertheless, that some of the traits in one of these groups do not entirely dissolve in this way. These traits are: Confidence, Realism, Strength of Personality, Tact, and Energy—in the sense given in the list above. The possibility of their being governed by a group factor—in addition to the '*w*' with which they are considerably saturated—merits a special investigation which we could not undertake in the present general research.

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[1] See pp. 54–55 of thesis (5).

Apart from specific connections between pairs of traits, therefore, which may be of interest and importance in other directions, *four* groups of traits—two clearly based on two general factors already recognized in a previous research and two arising from new, unconfirmed and more limited group factors—remain for our consideration. The traits in these groups are as follows, in approximate order of saturation with the group or general factor concerned in each. Those traits which have to be set aside from the pool on account of their sharing among them limited group or special factors are shown in brackets. The pool formed from the remainder should give a good measure of the general factor, since the numerous and varied specific factors, being approximately equally represented, will cancel out.

'*c*'	
+Traits	−Traits
Cheerful	Gloomy
Natural	Formal
Sociable	Unsociable[1]
(Humorous	Earnest)
Adaptable	Conservative
(Gregarious	Exclusive)
Quick of Apprehension	Slow of Apprehension
(Hasty	Introspective)
(Responsive	Unresponsive)
Forward	Shy
(Verbose	Taciturn)
(Optimistic	Pessimistic)
Co-operative	Distrustful
Original	Banal
(Realistic	Autistic)
Expressive	Inhibited
(Sensuous	Not Hedonistic)
Placid	Given to Mental Conflict
(Confident	Anxious)
(Tactful	Tactless)
Sophisticated	Naïve
(Contented	Ambitious)

'*w*'	
+Traits	−Traits
(Conscientious	Unreliable)
Persistent	Changeable
(Persevering	Wilfully Changeable)
Energetic	Inert
Tactful	Tactless
(Emotional Mood Steady	Emotional Mood Oscillating)
Mature	Wilful
Pedantic	Casual
Not Hedonistic	Sensuous
Confident	Anxious
Kind on Principle	Not so
(Realistic	Autistic)
(Introspective	Hasty)
(Good-natured	Malicious)

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[1] It is perhaps fitting to remind the reader at this point that these labels have precise connotation and that he may not put any generalized meaning he likes upon them. Sociability, for example, means readiness to make contacts, and it is perhaps significant that it makes a higher correlation with '*c*' than does Gregariousness—the mere liking to be in large groups.

'*m*'		'*a*'	
+Traits	−Traits	+ Traits	−Traits
Mature	Wilful	Balanced	Extreme
Good Natured	Malicious	Frank	Secretive
Kind on Principle	Not so	Optimistic	Pessimistic
Tactful	Tactless	Generous	Stingy
		Objective	Subjective
		Tactful	Tactless
		Co-operative	Distrustful
		(Emotional	Unemotional)[1]
		(Natural	Formal)[1]

The last two group factors have been temporarily given the names 'maturity' and 'adjustment' and represented by the letters '*m*' and '*a*,' for the former is found in a group of qualities which tend to come with maturity, whilst the latter underlies a set of qualities which in clinical practice are commonly regarded as characterizing the well-adjusted rather than the maladjusted individual. Nevertheless, it must not be overlooked that these two group factors cannot be regarded as established, for specific intercorrelations have been shown to exist, by means of tetrad differences, only in a majority, not in all, of their possible connections. There is, too, more than a suspicion of a connection between '*m*' and '*a*,' though not sufficient to justify lumping all the traits together.

An attempt was made to get further evidence for the existence of these factors by eliminating progressively from among the traits with which they are associated all intercorrelation due to '*c*' and '*w*.' Four traits from '*m*' and the four first traits of '*a*' were selected to be put to the test. Their correlations with the '*c*' and '*w*' pools are as follows:

Table II. *Correlation of traits in* 'm' *and* 'a' *group with* 'c' *and* 'w.'

	Mature	Good Natured	Kind on Principle	Tactful	Balanced	Frank	Optimistic	Generous
'*c*'	0·06	0·00	0·02	0·16	0·33	0·34	0·38	0·37
'*w*'	0·62	0·42	0·53	0·66	0·15	0·07	0·13	−0·02

Throughout this and other tables the lowest coefficient which exceeds three times the P.E. is 0·25.

When, by partial correlations, the influences of '*c*' and '*w*' on the intercorrelations of these traits have been successively eliminated, the following intercorrelations remain:

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[1] These traits are left out of the pool because although they show specific correlations with some of the other members they actually have one or two negative correlations with others which may or may not be due to chance errors of estimate.

Table III. *Correlation of* 'm' *and of* 'a' *traits when* 'c' *and* 'w' *have been partialled out.*

'm' *traits.*

	Mature	Good Natured	Kind on Principle	Tactful
Mature	—	—	—	—
Good Natured	0·29	—	—	—
Kind on Principle	0·12	0·59	—	—
Tactful	0·27	0·03	0·22	—

'a' *traits.*

	Balanced	Frank	Optimistic	Generous
Balanced	—	—	—	—
Frank	0·45	—	—	—
Optimistic	0·38	0·38	—	—
Generous	0·33	0·16	0·27	—

Evidently the '*a*' factor is a substantial entity and probably unitary, but the '*m*' factor may dissolve into a number of more limited specific correlations. Moreover, if the correlation of the '*a*' pool with the '*m*' pool (see table VI below) is corrected similarly for the effect of '*c*' and '*w*' a correlation of 0·31 remains. Possibly, with the transfer of certain '*m*' qualities to the '*a*' list, the former would dissolve into mere pairs of specifically related traits, but a further research to confirm and augment the reliabilities of the present estimates and correlations would be desirable before setting out to investigate these finer connections.

It is a matter of interest at this point to determine exactly to what extent the traits in the '*c*' and '*w*' groups are saturated with '*c*' and '*w*,' and also to discover what the correlations between certain representative traits would amount to if the attentuating effects of chance errors of estimation were allowed for. For this purpose representative traits were selected from the top, the middle and the bottom of the '*c*' and '*w*' lists.

Table IV. *Correlation of some representative Surgent and Will Character traits with* 'c' *and* 'w.'

	Correlation with the '*c*' factor	
(Top of list)	Cheerfulness	0·89
(Middle)	Quickness of Apprehension	0·55
(Bottom)	Placidity	0·11
	Correlation with the '*w*' factor	
(Top of list)	Conscientiousness	0·80
(Middle)	Steadiness of Emotional Mood	0·69
(Bottom)	Confidence	0·22

Table V. *Intercorrelations of some Surgent and Will Character trait estimates when corrected for attenuation.*

(Top of list)	Cheerful—Sociable	0·98
(Middle)	Quick of Apprehension—Forward	0·56
(Bottom)	Placid—Sophisticated	(Negative)
(Top of list)	Conscientious—Persistent	0·96
(Middle)	Emotional Mood steady—Mature	0·39
(Bottom)	Confident—Kind on Principle	0·28

We appear to have obtained from our correlation tables evidence of independent '*c*' and '*w*' factors such as have been found in previous research. It remains to be seen whether this independence is borne out by the correlation of the pooled '*c*' and the pooled '*w*' trait estimates. The intercorrelations of the four factors concerned, obtained by a straightforward, unweighted pooling of the traits in the above list, are as follows:

Table VI. *The relationships of Surgency, Will Character, 'Maturity' and 'Adjustment' as determined by correlation of pooled traits*[1].

	'*c*'	'*w*'	'*m*'	'*a*'
'*c*'	*0·63*	−0·03	0·12	0·52
'*w*'	−0·03	*0·64*	0·48	0·38
'*m*'	0·12	0·48	*0·76*	0·45
'*a*'	0·52	0·38	0·45	*0·75*

A clear confirmation of the independence of '*c*' and '*w*' results. It is noticeable that both '*m*' and '*a*' correlate positively with '*c*' and '*w*' and with each other. Since the traits involved already contain '*c*' and '*w*' (whatever further factors they may contain) this does not surprise us. The fact that the 'reliabilities' of these factors are even greater than those of '*c*' and '*w*,' however, again suggests that something over and above '*c*' and '*w*' is genuinely possessed in common by the traits of each group. This agrees with the result obtained above by partial correlations.

V. DISCUSSION AND SUMMARY: THE SURGENT TEMPERAMENT.

Since the present findings confirm and extend those of earlier research, as far as '*c*' and '*w*' are concerned, there is every reason to regard these two factors and the resultant pair of type opposites as established, at least in terms of estimates.

But it remains to discuss whether we are entitled to apply the term 'temperament' to either of them, if temperament is to be understood as physiologically determined, for we have established no physical or physiological connections. No one would think, however, of calling the '*w*' factor 'temperament,' for it is clearly that which is referred to in everyday life as will, consistency of behaviour, integration of character, and indeed as character in the broadest sense of the word. Now, whatever the theory of temperament may be, it has certainly been the practice of psychologists to describe as temperament any broad behaviour pattern which is not due to character. Consequently we shall not err from the usual convention by calling '*c*' a temperament factor—perhaps *the* temperament factor—since it covers a so much wider variety of reactions than does '*m*' or '*a*.'

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[1] The 'reliabilities' are calculated as the correlations between all the odd traits as a whole and all the even traits in the pools. The P.E. is approx. 0·08 for the smaller correlations and less for the greater ones.

A promising field for further research opens out here. There is the task of filling out the list of traits which are largely dependent upon '*c*'; of elucidating the relation of '*c*' to '*a*' and to other more restricted and specific factors; and, finally, of investigating the possibility of these factors depending upon physical and physiological conditions.

At this point we may notice that the '*c*' temperament pattern is not that of the extravert, the cyclothyme or indeed any other type so far described and labelled. True, it has some resemblance to the extravert type, by which it is approached more closely than by any other, but there are some significant differences. Even if these could be overlooked, it would be inviting confusion to take over one of these approximate terms, which are already employed with fixed and precise meaning in a very considerable literature. The empirical origin and experimental associations of the '*c*' factor justify and require a new, specific and unworn terminology.

In an article continuing the present one it will be shown that the mental process behind the '*c* +' mentality is a fluency of association (high in actual tests), a quickness of response and a ready 'leaping up' of ideas and impulses. A glance at the traits in the '*c*' group is alone sufficient to indicate that the essence of the temperament is expressible by some term conveying the idea of 'leaping' or 'rising up' with facility. 'Repressed' and 'Unrepressed' convey more than we are entitled to infer at present. The terms Sthenic and Asthenic are perhaps nearer, but they have already been used with definite intent and we may not safely assume at present that the difference is one of greater or less strength or energy of mind. The word 'Surgent,' from the Latin *surgo*, seems most aptly to express the quality which the tests reveal. The opposite temperament, possessing little '*c*,' could then be conveniently described as the 'Desurgent' temperament (this is not etymologically the true opposite, but it has that quality in English), and the amount of '*c*' possessed by any individual could be referred to as the degree of Surgency.

Considerable interest attaches to the '*a*' factor, which, as we have seen, survives the elimination of '*c*' and '*w*' and possibly absorbs '*m*' to itself. A survey of the traits with which it is most definitely associated reveals at once its strong resemblance on the positive side to the cyclothyme type. Six of the eight traits in the above '*a*' list practically correspond to six of the eight traits which Sahai (19) showed to be most characteristic of the cyclothyme temperament (*Natural*, *Generous*, *Co-operative*, *Frank*, *Balanced*, *Emotional* (corresponding to *Sociable*), *Warm-hearted*, *Trustful*, *Tendency to enjoy gifts of life*, and *Varying in Emotional Mood*). Objective and Optimistic, which never came up for consideration in Sahai's abbreviated list, are nevertheless among the qualities assigned to the cyclothyme by Kretschmer (17). On the other hand, there is a definite absence of Humorous, one of Sahai's eight highly intercorrelating cyclothyme traits, from our '*a*' list, although it was among the qualities given for estimation. A good part of Sahai's correlations was undoubtedly due to the '*c*' factor which underlay many of the qualities in his list (he recognized the existence of two group factors running through the qualities), but there was an especially high correlation among these particular cyclothyme traits due to the additional limited group factor. A further indication of the identity of cyclothymeness and '*a*' will be found in the ensuing article, where our results show that of all the factors isolated from our estimates the '*a*' factor has the highest positive relation to an index of the pyknic body build. (The correlation is a very low one, but so too was that found by Sahai, employing a much more complete and carefully controlled measure of the pyknic build than we could hope to use in this research.)

To sum up: the research has revealed, in addition to a General Character Factor, '*w*,' and a broad Surgent-Desurgent temperament type distinction, '*c*,' a reasonably clear indication of the existence of a schizothyme-cyclothyme type distinction depending upon an '*a*' group factor over and above the general '*c*' factor.

These generalizations cannot safely be allowed to rest on a basis of estimates alone: it will be necessary to discover the exact limits of the traits concerned in these estimates and just how the thing estimated expresses itself in test situations or precise records of behaviour. The work of Hargreaves (10), for example, points to the conclusion that Originality as estimated is not the same thing as originality in test situations: it is rather an estimate of how frequently and easily the individual is original than how profoundly he is original. It is such points as this which urgently need investigation if the gains resulting from the method of estimates are to be consolidated.

The second direction in which enquiry is particularly needed is that of the variation of patterns with age. Is the Surgent pattern the same in childhood as in adult life? Is the schizoid child also schizoid in adolescence, and if so is the emphasis on the various component traits just the same or do fresh characteristics now distinguish the type? Sahai, in the above-mentioned research, found that correlation tables obtained from estimates in two schools correlated together to the extent of 0·98, but only to the extent of 0·50 and 0·51 with correlations of estimates on the same qualities made in a training college of adolescents. There is evidently room for special enquiries here.

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PUBLICATIONS RECENTLY RECEIVED.

The Psychological Register. Vol. III. Edited by CARL MURCHISON and others. Clark University Press (London: Humphrey Milford). 1932. Pp. ix + 1269. 56*s*. 6*d*. net.

This issue of the *Psychological Register* practically supersedes the 1929 edition which is henceforth to be known as Vol. II. Vol. I will contain records of earlier psychologists and will "extend all the way back to early Greek psychologists." It is also hoped to publish a fifth volume containing a "cumulative index" with the names of "all the important psychologists that have ever lived." Vol. III represents 40 countries and contains the names, records and bibliographies of about 2400 psychologists. It is a monument of thorough and careful work, the editing of which must have involved a prodigious amount of labour. The result of this work is indeed almost staggering in its bulk and completeness. No psychological reference library can afford to be without the new Register, even though at the same time probably few psychological reference libraries can really afford to purchase it. If there is one doubt about this very valuable enterprise it is whether the publication of exceedingly detailed bibliographies is really necessary. Some of the contributors have taken the bibliography matter very literally indeed and have sent up pages and pages of references. It is well to have them all together perhaps; but maybe a curtailed edition, with general bibliographical indications only, would be found very nearly equally useful, as it would assuredly be found much cheaper to produce.

Vision and Colour Vision. By R. A. HOUSTOUN. London: Longmans, Green and Co. 1932. Pp. vii + 238. 15*s*. net.

Dr Houstoun has written an admirably competent, clear and interesting book on Vision, which should be in every psychological library. The historical treatment of the subject is not exhaustive, but the most important and reliable experimental work is described, in each case with a detailed account of the methods and apparatus used, and in addition the experimental work of the author and his collaborators on discrimination of intensity, acuity, colour mixing, simultaneous contrast and colour blindness. The last piece of research is particularly valuable in that it was carried out on a large number of subjects, not merely upon one or two, thus avoiding a criticism of technique which the psychologist might level against much of the work reported. The results and theories described have been worked out in full mathematical detail. Dr Houstoun brings much convincing evidence to bear against the duplicity theory of V. Kries, and against the Young-Helmholtz theory of colour vision; for the latter he would substitute a modified form of Hering's theory.

The Psychology of Study. By C. A. MACE. Methuen's Monographs on Philosophy and Psychology. London: Methuen and Co., Ltd. 1932. Pp. viii + 96. 2*s*. 6*d*. net.

This witty and pleasantly written little book may be recommended to anyone who desires to learn what the psychologist can teach him as to how to direct his studies and employ his mind so that he may eventually hope to attain to the power of original thought. Mr Mace gives much valuable advice for the training of observation, memory and concentration, but shows that to be of any use they must necessarily subserve interest: "the ultimate source of efficiency in observation, in memory, and in constructive thought is insatiable curiosity and the will to know."

We may, however, be able to learn a great deal about the direction of these motives, within their natural limits; and even the trained thinker may find something in Mr Mace's book of interest and profit to himself.

Mind and Money: A Psychologist looks at the Crisis. By JOHN T. MACCURDY. London: Faber and Faber. 1932. Pp. 319. 10*s.* 6*d.* net.

This book is divisible into three parts. Chapters I–IX deal with psychological characteristics underlying the formation of groups; chapters X–XII concern the psychological factors behind economic facts—such as comforts and luxuries, money and credit; and the remainder of the book analyses the psychology of Germany, America and Great Britain, giving predictions about their probable future development. In many parts it is brilliant in its insight. Doubts are thrown on the wisdom of quantity production; comforts and luxuries are analysed psychologically in a manner that should prove useful to the economist; and it is made clear how the wireless and the press tend to form a psychological link between people widely separated in space, with the result that their actions may then become similar to those of a crowd linked by spatial contiguity. The book has a very definite point of view which is put by the writer with force. Dr MacCurdy endeavours to show that the aristocratic form of organization is the only form which will ensure stability in a group, but he does not make it clear whether he means aristocracy in the sense of hereditary privilege or in the sense of government by experts. Dr MacCurdy thinks that the future is black for Germany and worse for the United States but that England will pull through all right in the end. He misses a good opportunity in this section of dealing seriously with the probable future of Russia.

The Evolution of Human Behaviour. By CARL J. WARDEN. New York: The Macmillan Co. 1932. Pp. ix + 248. 15*s.* net.

The book covers an enormous ground in a relatively untechnical way. The author has, however, an extremely good knowledge of his field and shows excellent powers both of selection and of arrangement. After a general statement of some of the chief problems of human evolution, the book goes on to deal with the main changes that marked the rise of man; with the traces left by very early man; with "the coming of modern man"; and concludes by two essays on "race and civilization," and "present trends in evolution." A student must, of course, read cautiously in a subject which affords great scope for speculation and is at the same time one in which new information is being rapidly acquired. But, with this condition in mind, the volume is an excellent one to put into the hands of the student of psychology. It will raise many fascinating problems for him, give him plenty of clues towards their solution, and perhaps stimulate him to attempt a more psychological study of the vast field of research which is described.

Instinct, Intelligence and Character. By GODFREY H. THOMSON. 2nd edition. London: Geo. Allen and Unwin, Ltd. 1932. Pp. 282. 10*s.* 6*d.* net.

Prof. Godfrey Thomson's excellent book is so well known that it stands in need of no lengthy review. That successive impressions and now a second edition should have been called for rapidly is very encouraging to all those who are interested in a sane, thoughtful and well-informed development of psychology. The general theme of the book is that it is easier to guide the development of character than to guarantee a certain level of intelligence. As Prof. Thomson puts it: "It seems to be the function of the teacher to form character and find out about intelligence." The theme is not supported merely by general argument, but by particular, and attested, facts, and Prof. Thomson is not only concerned to say what should be attempted, but also to indicate as precisely as possible how to succeed in the attempt. While the book is addressed largely to the teacher, it has a very wide scope and can be read with pleasure and profit by anybody who finds an interest in following the growth of mental life. In every way this is a good book, and in future it should be yet more widely studied.

Emotional Currents in American History. By J. H. DENISON. New York and London: Charles Scribner's Sons. 1932. Pp. vii + 420. 21*s.* net.

This ought to be a good book, for it has a capital subject to study. Actually it is very diffuse, it is most irritating in its strong bent for 'purple patches' and 'fine writing,' and nothing very definite emerges out of it all except a rather confused notion that the apparent welter of chaotic custom and feeling which characterizes social conduct is actually the expression of a few strong and persistent emotional tendencies operating in a more or less regular ebb and flow. The sort of writing that the author loves is this: "We have beheld an Olympian Drama in which ancient spirits rising out of the misty Past have met with other spirits summoned from the vasty deep, and have fought for the Soul of America." All through there is a great deal too much of the attitude that if you write the name of anything with a capital letter you invest that thing with a sort of superior reality and value. With all this any reader who can stomach the exaggerated and diffuse style of writing may get from this book many interesting pictures of the course of American social history. But he ought to compare them with representations of the same facts in some more sober history.

La Determinazione del Presente Psichico. By RENATA CALABRESI. Firenze: R. Bemporad e Figlio. 1930. Pp. xi + 188.

This is a very capable monograph which presents an account of a comprehensive research carried out under the main direction of Prof. Bonaventura. The author shows a wide knowledge of earlier work on the psychic present, especially that done by German investigators, and she summarizes this work clearly and adequately. Her own experiments were of the tachistoscopic kind and in the line of direct development from the early work of Wilhelm Wundt. It is maintained that the psychic present represents a genuinely concrete experience, the objective limits of which vary to some extent from person to person and from one content to another. Broadly speaking objects presented in actual succession, with limits, roughly, of 800σ to 1000σ, are synthesized by a cognitive act and treated as belonging to a single experienced moment. Certain epistemological conclusions are drawn which hardly seem to be necessary. The laboratory psychologist will be interested in the design of a double tachistoscope for this work, by Prof. Bonaventura.

Ten Years of Industrial Psychology: An Account of the First Decade of the National Institute of Industrial Psychology. By H. J. WELCH and C. S. MYERS. London: Sir Isaac Pitman and Sons. 1932. Pp. ix + 146. 6*s.* net.

When, some day, the full history of the development of scientific psychology in this country comes to be written, it will be seen clearly that, in an outstanding number of directions, the main growth has been due to the vision and enthusiasm of one man—Dr C. S. Myers. In Industrial Psychology, as in numerous other developments, he has played a predominant part. This book gives a clear and deeply interesting account of the foundation and growth of the National Institute of Industrial Psychology, in the work of which Dr Myers was from the first associated with Mr H. J. Welch, and, for the greater part of the time most ably aided by the present Director, Dr G. H. Miles. It is a record of astonishing and consistent success in many fields. The story of its inception, its rapid growth in spite of persistent industrial depression, and its multifarious activities, is told well and concisely, and constitutes a record of public and scientific achievement of which anyone could legitimately be proud. The volume should receive a wide welcome, for it will provide a stimulus to renewed efforts on the part of psychologists everywhere towards the development of their science.

The Experimental Study of Reading. By M. D. VERNON. Cambridge University Press. 1931. Pp. xv + 190. 8*s.* 6*d.* net.

In one respect at least most people who write about psychological problems could learn a lot from a study of this book. It contains no superfluous words, and every bit of it is to the point in the most concise and clear manner. All the important problems concerning the methods of study of eye movements and their functions in reading are considered, and there is some discussion of relevant points in the psychology of visual perception with special reference to the visual perceptual processes of young children. A brief, but comprehensive study of typographical factors in relation to reading is included. The whole book reaches a high and sustained level of critical exposition and a very considerable part of it is obviously the outcome of first-hand experimental investigation which was well planned and carefully conducted. If there is to be any adverse criticism it must be on the ground of a certain severity, both of expression and of outlook, which perhaps makes the author unduly loth to take risks with any conclusions except predominantly negative ones. But that only strengthens one's confidence in such positive results of investigation as are stated with approval. The book has a wide appeal, to the general psychologist, to the educationist, to the laboratory worker, and to the typographer. It deserves and indeed demands close and detailed study, and ought to be very largely read. A valuable selected bibliography is appended.

The Measurement of the Intelligence of Young Children by an Object-Fitting Test. By RUTH ELLEN ATKINS. Minneapolis: University of Minnesota Press. 1932. Pp. 89.

The test consists in fitting various common and readily recognized objects into appropriately shaped depressions in papier-mâché trays. Full details are given for the construction and of the standardization of a series of such tests. It is claimed that the tests are of particular value in the case of young children, of deaf children, and of children who do not understand the language of the experimenter.

Certain Relationships between Scholarship in High School and in College. By LENA J. HAWKS. The Johns Hopkins University Studies in Education, No. 15. 1931. Pp. viii + 58. $1.15.

The Reliability of Quarterly Marks in the Seventh Grade of Junior High School, together with the value of Certain Standard Tests in Predicting them. By J. CAREY TAYLOR. The Johns Hopkins University Studies in Education, No. 17. 1931. Pp. ix + 54. $1.15.

A Series of Experiments on the Learning of French Vocabulary. By LOUISE C. SIEBERT. The Johns Hopkins Studies in Education, No. 18. 1931. Pp. 106. $1.50.

Of these three monographs No. 18 is of the most general interest. It records a capable experiment in mental learning and discusses, in terms of the experiment, most of the main experimental problems which are commonly raised in regard to learning in the field of mental work.

The Moral Judgment of the Child. By JEAN PIAGET. London: Kegan Paul, Trench, Trubner and Co., Ltd. International Library of Psychology, etc. 1932. Pp. x + 414. 10*s.* 6*d.* net.

Prof. Piaget first studies "the rules of the game," on the ground that in simple social games played by children we "are in the presence of rules which have been elaborated by the children alone." The contention could be criticized but nevertheless the author's analysis of the rules of a game of marbles as it is played in Geneva and Neuchâtel is interesting. Whether marbles as it is there played is a particularly suitable game to choose as typical is perhaps another matter. Thereafter Piaget pursues his well-known conversational method, putting little concrete moral problems to children of varied ages and carefully giving both the manner and the matter of their conversational replies. The general conclusion is that the child has two separate moralities: one due to adult moral constraint, leading to moral realism, and the other to social co-operation, leading to autonomy. "Between the two can be discerned a phase during which rules and commands are interioralized and generalized." Rather less than half the book leads up to these conclusions, and the rest of the volume is occupied by a deeply interesting discussion of the social significance of developing moral sanctions and the relation of this to what Piaget calls 'rational consciousness.'

In spite of much diffuseness of expression which often blurs the definition of the argument, Prof. Piaget has produced a book of value and significance. A lot of the material is necessarily controversial but he makes his points with fairness and unfailing good humour.

The Reliability of Examinations. By C. W. VALENTINE. London: University of London Press. 1932. Pp. 195. 7*s.* 6*d.* net.

This is a volume of the very greatest interest and significance to all who are concerned with problems of educational administration and especially of the distribution of educational rewards. It represents the result of several years' careful and painstaking collection of data concerning the subsequent *educational* careers of scholarship students in secondary schools, in English provincial universities, and in the foundations of Oxford and Cambridge. The results, save only in the case of Oxford and Cambridge, are profoundly disturbing and point to serious defects in the present system of scholarship awards. The detail of these results does not readily lend itself to a very brief presentation, but is set forth with great force, clarity, and much happy wit in this book. Reasons for the present state of affairs are discussed, and a number of interesting suggestions for amendment are made. The book can be recommended without reserve. It is a significant social and psychological study in performing which Prof. Valentine has done conspicuously able service to the development of educational selection in this country.

Die Spaltung der Persönlichkeit: Deutsche Übersetzung und Bibliographie. Von WILLY HERMS. Stuttgart: W. Kohlhammer. 1932. S. xvi + 270. R.M. 16.

This is the ninth volume of a series edited by Prof. T. K. Oesterreich dealing mainly with problems of personality. It contains excellent translations of a description by the late Dr Morton Prince of the famous case of Sally Beauchamp, and of Dr Walter F. Prince's detailed studies of Doris Fisher and Heinrich Meyer. The translator adds a full and well-chosen bibliography of studies in divided personality. A brief introduction by Prof. Oesterreich gives details of the careers and work of Dr Morton Prince and of Dr W. F. Prince.

Mental Deficiency Practice. By F. C. SHRUBSALL and A. C. WILLIAMS. London: University of London Press. 1932. Pp. vii + 352. 12*s.* 6*d.* net.

This is a book which will be found of great value by the physician, the social worker, the practising psychologist, or anyone else who has to deal with concrete problems of mental deficiency. The clinical aspects of the problem are to the fore throughout, and excellent accounts are given of clinical examinations for the mentally defective, of the use of mental tests, of the general lines of diagnosis, and of the kinds of effects of treatment that may be expected. Special sections deal with legal aspects of the problem involved in so far as English Law is concerned, with the work of Local Education Authorities and with the duties of Local Mental Deficiency Authorities. An excellent range of specimen official forms is given. The book is in fact a practical manual which should speedily come to occupy an authoritative position in literature of its kind. Moreover, it is written very clearly, so that all who read may understand.

Individual Psychology. Medicine and the Bases of Science. By F. G. CROOKSHANK. London: C. W. Daniel Company. Individual Psychology Publications, No. 3 *a*. 1932. Pp. 71. 2*s*. 6*d*. net.

Under the general title, *Individual Psychology and Medicine*, the author presents four short essays on: Diagnosis; the Neurotic Character; Understanding Human Nature; the Patient. A further essay deals with Individual Psychology and the Bases of Science. A list of English books dealing with Individual Psychology is included.

A Note on the History of the Care of the Insane in Ireland. By T. P. C. KIRKPATRICK. Dublin: The University Press. 1931. Pp. 43.

This pamphlet gives a concise and interesting account of the methods of treatment of insane persons in Ireland from very early times to the end of the nineteenth century. It is the fruit of an immense amount of careful research, and as an important chapter in the history of the development of public opinion and private understanding in the treatment of mental abnormalities is of permanent value.

An Introduction to Analytical Psychotherapy. By T. A. ROSS. London: Edward Arnold and Co., Ltd. 1932. Pp. viii + 203. 10*s*. 6*d*. net.

A second book on psychotherapy from the author of *The Common Neuroses* is very welcome. Whereas *The Common Neuroses* dealt with therapy by persuasion, this work treats of therapy by analysis, so that it is in reality a second volume of a two-volume work on psychotherapy, though there is a certain amount of overlap.

This volume is essentially practical, written by a practical man of great experience who possesses the ability to say what he wants to say clearly and concisely. Dr Ross examines three questions: (1) Does an unconscious mind exist? (2) If it does, can it be explored? (3) Does benefit accrue from its exploration? It is obvious that the answers reached, after the discussion of much first-hand material, must be in the affirmative; but that is a comparatively minor point. The main interest is in the way in which the evidence is obtained.

Besides the discussion, in an unorthodox manner, of such orthodox subjects as the Unconscious, Transference, Dreams, Some Sex Problems, etc., a detailed study of one individual case is made. This, with the later chapter on Technique, gives an illuminating insight into Dr Ross's methods. Throughout run the general principles of Freud, but they are modified, the psycho-analyst would maintain, so that they are no longer Freudian. This is true; but no good work can be done, especially in psychotherapy, by one who merely follows in another's footsteps.

An Introduction to Analytical Psychotherapy should be in the hands of all who are interested in this branch of the art of healing.

What we put in Prison. By G. W. PAILTHORPE. London: Williams and Norgate. 1932. Pp. 159. 5*s*. net.

Studies in the Psychology of Delinquency. By G. W. PAILTHORPE. Medical Research Council Special Report Series, No. 170. London: H.M. Stationery Office. 1932. Pp. 113. 2*s*.

These two very important works by Dr Pailthorpe to a great extent cover the same ground. The detailed case histories in the one are repeated almost word for word in the other. The book contains more general discussion, however, while the Report is more closely confined to the specific investigation which the author carried out. The investigation, which consisted of very thorough and careful personal interviews, supplemented by equally carefully collected general information, dealt with a large group of prison cases. Dr Pailthorpe found that her cases could be classified under three main headings: the defective group, the psychopathic group and the adapted group. The psychopathic group contained cases of mental conflict and cases of psychotic disorders. It is made clear that the groups overlap. Many detailed case histories are given, and the bearing of these, both on educational problems and upon criminal administration is discussed with great clarity and force. Both the book and the Report are worthy of careful and detailed study, and the author is to be congratulated on a contribution to the psychology of the development and treatment of criminal tendencies of great moment.

Caligula. By H. SACHS. Translated by H. SINGER. London: Elkin Mathews and Marriot. 1931. Pp. 224.

The publishers state that this is "a brilliantly written psychological study of one of the most extraordinary pathological characters known to history." This is an exaggeration, but nevertheless the book is one of great interest and it is much less unpleasant than perhaps many people will hope.

Early Beliefs and their Social Influence. By EDWARD WESTERMARCK. London: Macmillan and Co. 1932. Pp. 182. 7*s.* 6*d.* net.

This is the substance of lectures delivered at the London School of Economics. It is an attempt to trace the influence of early custom and belief, particularly those clustering about magic and religion, upon various departments of social life and organization. After a brief discussion of the characteristic differences between magic and religion, Dr Westermarck deals in turn with their influence upon social practices connected with charity, hospitality, the place of children in society and the regard for truth, the growth of justice, duties to gods, marriage and sexual relations, and the position of women. Though there is nothing strikingly new in this work, a mass of evidence is skilfully and clearly arranged, and the book provides the social psychologist with important problems and data.

Set the Children Free. By FRITZ WITTELS. Translated by E. and C. PAUL. London: Geo. Allen and Unwin, Ltd. 1932. Pp. 242. 10*s.* net.

The author's ideas of the mental life of the child are derived mainly from Freud to Lévy-Bruhl, and he finds these ideas confirmed in his own first-hand observations. After a general discussion he proceeds to deal with a number of specific problems mainly from the point of view of the relationship of parent and child. His main questions concern the methods of satisfying children's curiosity: the correct regulation of punishment; the influence of the relationship of father and mother on the child, and of the child on the relationship of father and mother; nursery management and the child's subsequent career; self-defence and imagination in the child; stepchildren; and the position of the children of divorced parents, of illegitimate children, and of orphans. A final chapter contrasts recent educational movements in Germany and Austria with the present position in England. The book demands discrimination in its study, but taking it as a whole, it is an excellent and timely treatment of a number of difficult and pressing problems.

Diseases of the Thyroid Gland. By CECIL A. JOLL. London: Heinemann. 1932. Pp. xviii + 682. £3. 3*s.* 0*d.*

This finely produced and printed book is especially addressed to senior students and practitioners. It contains a full and clear account of diseases of the thyroid apparatus and their treatment. The anatomy and physiology of the thyroid gland are treated at considerable length and with particular care. There is a very full discussion of thyrotoxicosis, its diagnosis, causation and treatment. The volume is the outcome mainly of direct observation and clinical experience, but well-selected references to the vast mass of the existing literature are given. Few psychologists may be able to give this book the really detailed reading and study that it deserves, nevertheless the volume is well worthy of the psychologist's attention. In a field somewhat notable for wild speculation it remains strictly tied to scientific fact, and it gives many well-observed and recorded cases of the repercussions of glandular disturbance upon the temperament and character of the patient. Its twenty-four colour plates and two hundred and eighty-three figures are beautifully reproduced.

The Maniac. ANON. London: Watts and Co. 1932. Pp. xix + 259. 7*s*. 6*d*. net.

This is the second edition of a book, first published in 1909, which purports, and is guaranteed by the publishers, to be "a realistic study of madness from the maniac's point of view." It gives a full and detailed account of the obsessions and delusions occurring during an attack of acute mania, as they were recollected and set down by the maniac on her subsequent recovery.

An Introduction to Pneumatology. By J. C. McKERROW. London: Longmans, Green and Co. 1932. Pp. 179. 6*s*. net.

Pneumatology is defined as "the study of man, the religious animal, or, an expression we shall use as synonymous, a spiritual being." This book therefore is a series of essays on the psychology of religion. And the essays are undoubtedly worth reading, for they contain much serious thinking and are written in an incisive and attractive manner. The author believes that the spiritual being 'emerges' when the 'natural animal' has become aware of the limitations of reason. He endeavours to describe the mode of this 'emergence' in some detail, and then proceeds to consider the pathology of religion, revelation, contrition, and 'intention' which is the direction of an action "performed solely for the glory of God, without the alloy of any human motives." A great part of the book is necessarily controversial in character, and many of the author's rather blank dogmatisms may arouse opposition on the part of the reader, but he has succeeded in producing a stimulating volume.

Les pouvoirs inconnus de l'esprit sur la matière. Par Dr EUGÈNE OSTY et MARCEL OSTY. Paris: Félix Alcan. 1932. Pp. 153. 25 francs.

This volume is an account of the experiments conducted by the director of the Institut Métapsychique, in Paris, with the help of his son. The subject of the inquiry was the young Austrian medium, Mr Rudi Schneider, whose alleged 'supernormal' powers have been the centre of heated controversy for some years. Among his supposed psychic faculties is the power of moving objects at a distance without any normal contact. This surprising phenomenon is said to be exercised under rigid conditions of control, although the evidence for this has been contested.

In the present series of experiments proof of these faculties is sought by means of an installation whereby the passage of infra-red rays, when interrupted, controls an electric circuit, which in turn actuates the registration instruments.

The medium, having entered into a condition roughly designated as trance becomes 'controlled' by a secondary personality, or *soi-disant* spirit, who directs the operations. The psychic force is then extruded by the medium towards the field on which the infra-red rays have been directed, and by their interruption at varied intervals the influence at a distance is demonstrated and recorded on photographic paper. The records of the medium's hyperpnœa (180 to 400 per minute) are interesting when compared with the chronological series of infra-red interruptions, which take place, not only at the direction of the medium's 'control, but also at the will of the observers.

If valid, it is clear that these experiments are of some considerable importance. Indeed it would appear that, assuming all sources of error be excluded, they offer a new field for intensive research. It is, however, difficult to understand why MM. Osty did not call for independent verification whilst the experiments were in progress. Without some such control it will not be possible to persuade the scientific world that Mr Rudi Schneider's powers are what is claimed for them. Moreover it is unfortunate that the record of the medium himself is open to grave criticism. Although something of his history has been printed in the present volume, the reader will look in vain for an account of those incidents in his career which have led many competent observers to cherish serious doubts concerning the reality of his alleged faculties.

Harold Høffding: In Memoriam. Copenhagen: Gyldendal. 1932. Pp. 112.

This memorial volume contains articles by Edgar Rubin, J. Jørgensen, V. Kuhr and F. Brandt, which deal both with Høffding as a man and with his psychological and philosophical work. A complete bibliography of Høffding's published works, by Kalle Sandelin, is included.

Causality. Edited by GEORGE P. ADAMS, J. LOEWENBERG and STEPHEN C. PEPPER. University of California Publications in Philosophy. Vol. XV. 1932. Pp. 231. $3.00.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

GENERAL MEETINGS.

November 15, 1932.	Business Meeting. (Followed by Lecture. See below.)
December 1, 1932.	Discussion Meeting. "Adaptation and Fatigue." Opening speaker: Prof. F. C. Bartlett, F.R.S.

LECTURES.

November 15, 1932.	"The Borderland between Education and Industry." By Mr A. H. Seymour, B.Sc.

SECTIONAL MEETINGS.

Medical.

October 26, 1932.	"The Technique of Group Psychological Work with Children." By Dr Margaret Lowenfeld.
November 23, 1932.	"The Psychology of Law-Making." By Miss Margaret Fry.

Education.

October 3, 1932.	"A Borstal Experiment in Vocational Guidance." By Mr Alec Rodger, M.A.
November 7, 1932.	"Estimation of Character." By Mr J. Wedeck, B.Sc.

Industrial.

September 14, 1932.	"Colour Discrimination in Industrial Processes." By Mr W. O'D. Pierce.
November 9, 1932.	"The Place of Commercial Entertainment in the Worker's Life." By Mrs Q. D. Leavis; Mr Oliver Raphael.

Aesthetics.

October 21, 1932.	"Poetry on Poetry." By Mr Charles Williams.
November 18, 1932.	"Artist and Scientist." By Dr H. Jennings White.

BRITISH PSYCHOLOGICAL SOCIETY

REVENUE ACCOUNT AND BALANCE SHEET AS AT 30 SEPTEMBER 1932

Revenue Account for the year ended 30 *September* 1932

30 Sept. 1931 £ *s.* *d.*	EXPENDITURE	£ *s.* *d.*	£ *s.* *d.*
– – –	*To* Rent, 55 Russell Square		154 11 7
57 10 0	Rent of Library prior to removal		35 0 0
200 0 0	Secretary's Salary		175 0 0
– – –	National Health and Unemployment Insurance		3 10 8
– – –	Salary paid to former Secretary		100 0 0
77 19 0	Printing		60 8 4
48 8 9	Hire of Halls		32 2 0
	Office and General Expenses:		
– – –	Caretaker, Lighting and Heating	31 3 5	
24 0 6	*Addressing*		
11 2 1	Stationery	31 3 6	
49 1 11	Postages and Telephone ...	55 3 4	
– – –	Fire Insurance	4 10 3	
6 16 6	Subscriptions to outside Associations...	6 16 6	
10 10 0	Audit Fee 1931	10 10 0	
16 13 7	Sundry Expenses, Fares, etc. (including Removal Expenses)	11 13 6	151 0 6
	Journals Publication Expenses:		
	General	168 13 3	
	Medical	124 0 5	
345 11 0	Educational	94 3 4	386 17 0
7 14 1	Teas		7 12 10
5 11 4	Scottish Branch Expenses		6 10 9
3 4 0	*Birmingham Branch Expenses*		
17 10	Bank Charges and Interest on Overdraft ...		5 10 9
– – –	Diplomas of Honorary Membership ...		8 8 0
41 14 11	*Register of Members' Qualifications*		
– – –	Legal Expenses *re* Lease, 55 Russell Square		37 0 0
41 4 0	*Legal Expenses* re *B.J.E.P., Ltd.*		
62 9 3	Depreciation of Library		63 9 8
– – –	Depreciation of Office Furniture and Fittings		27 0 5
13 5 6	*Loss on redemption War Loan*		
1023 14 3			1254 2 6
190 11 4	Balance, being Surplus of Income over Expenditure		135 15 4
£1214 5 7			£1389 17 10

30 Sept. 1931 £ *s.* *d.*	INCOME	£ *s.* *d.*	£ *s.* *d.*
	By Subscriptions 1932	1287 16 0	
	Subscriptions 1931	3 12 0	
		1291 8 0	
1095 0 0	*Less* Resignations and Removals	256 7 0	1035 1 0
15 0	*Birmingham Branch Members*		
114 17 2	Interest on Investments		108 7 7
– – –	Bonus on Assented War Loan		26 9 0
3 13 5	Interest on Bank Deposits		5 8 7
– – –	Interest on National Savings Certificates sold		214 11 8
£1214 5 7			£1389 17 10

Balance Sheet as at 30 September 1932

30 Sept. 1931 £ *s.* *d.*	LIABILITIES	£ *s.* *d.*	£ *s.* *d.*
2 9 0	*Sundry Creditors*		10 14 1
38 12 0	*Subscriptions in Advance*		35 0 3
	Publications Reserve		
72 0 0	Balance as per last Balance Sheet		72 0 0
	Revenue Account		
	Balance as per last Balance Sheet	4001 4 8	
	Add Surplus of Year	135 15 4	
4001 4 8			4137 0 0
£4114 5 8			£4254 14 4

Note. *Stock of Journals.* There exists a number of past and present volumes of Journals in Stock, but owing to the possibility that a large proportion of these may never be sold, no valuation can be placed on such stock.

30 Sept. 1931 £ *s.* *d.*	ASSETS	£ *s.* *d.*	£ *s.* *d.*
254 18 11	*Cash at Barclays Bank Ltd.*		24 14 10
8 13 11	*Postage Stamps in hand*		5 8
	Investments at Cost		
	£100 New South Wales 5½ % Loan 1924–34	100 0 0	
	£1100 5 % War Loan 1929–47 Assented	1117 6 9	
	£1545. 5*s.* 4*d.* Inscribed War Loan 1929–47 Assented ...	1600 0 0	
	£300 5 % Conversion Loan 1944–64	318 9 9	
3035 16 6			3135 16 6
	Note. Market value of above Securities at 30 Sept. 1932 £3111. 10*s.*		
202 13 0	*Subscriptions in Arrear*		141 13 0
	Office Furniture and Fittings		
	Expenditure during year ...	270 5 5	
– – –	*Less* Depreciation	27 0 5	243 5 0
	Library		
	Balance as per last Balance Sheet	562 3 4	
	Add Expenditure during year	72 12 10	
		634 16 2	
562 3 4	*Less* Depreciation	63 9 8	571 6 6
	Monograph Publications		
	Grants made during year ...	100 0 0	
– – –	*Less* amount repaid	12 7 2	87 12 10
50 0 0	*Shares in the British Journal of Educational Psychology, Ltd., at Cost*		50 0 0
£4114 5 8			£4254 14 4

I beg to report that I have examined the foregoing Accounts with the Books and vouchers of the British Psychological Society and that I have obtained all the information and explanations I have required. In my opinion the Balance Sheet is properly drawn up so as to exhibit a true and correct view of the state of the Society's affairs according to the best of my information and the explanations given to me and as shown by the Books of the Society. The Securities appearing under the heading Investments have been verified by production of the Banker's Certificate.

T. BARRETT,
Incorporated Accountant.
63 EASTCOTE LANE,
SOUTH HARROW.
10 *November* 1932.

S. J. F. PHILPOTT,
Hon. Treasurer.

VOLUME XXIII APRIL, 1933 PART 4

THE RELATION OF ACTS AND CONTENTS OF CONSCIOUSNESS[1].

BY CHARLES S. MYERS.

WHEN, a few weeks ago, I received the honour of an invitation to address a Plenary Meeting of this Congress, I had already prepared for one of its sectional meetings a communication on the statistics of vocational guidance. This, however, seemed to me too limited in its scope to interest any general gathering of psychologists. And in looking about for a wider theme, I was inevitably influenced by the interest which I had recently been taking in the relations between mind and living body, as well as by the desire to choose a subject which might appeal to all, or to nearly all, the various 'schools' of psychologists. My choice, therefore, finally fell on the subject of consciousness; for this, even to the most extreme of 'behaviourists,' must appear deserving of study, however alien he may regard it to 'scientific' psychology. And I endeavoured to select some special aspect of this subject which would be likely to be regarded as suitable for debate—neither, for example, the functional or spatial relations of consciousness to living matter, nor the relative primacy or importance of conscious and unconscious mind (both of these being too highly controversial problems, dependent for their solution largely on the particular 'school' of psychology to which the psychologist belongs—and I belong to none), but the several kinds, or 'modes,' of consciousness and their interrelations.

Surely, so it seemed to me, we psychologists would all agree in recognizing three modes of consciousness—(i) the consciousness of mental activity itself, (ii) the consciousness arising from, and consisting in, the mental outcome of (conscious or unconscious) mental activity, and (iii) in some way closely related to the two foregoing—the affective consciousness. It is mainly the relation to one another of the first two of these three modes of consciousness to which I venture to direct your attention to-day—the relation between what are commonly called (*a*) the *acts* (or, to use a variant, but perhaps not quite equivalent, term, the

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[1] A paper read to the Tenth International Congress of Psychology at Copenhagen, August, 1932.

processes) of consciousness and (*b*) the *contents* (or correspondingly, the *products*) of consciousness, or the conscious 'material' on which conscious mental work may be performed. In the brief time at my disposal I shall only be able to allude cursorily to the important third, or affective, mode of consciousness—the *feelings*.

I thought, too, that we might all agree in what is meant by the 'acts' and by the 'contents,' 'products' or 'material' of consciousness. On the one hand, we readily recognize the conscious *acts* of apprehending, relating, recalling, etc.; on the other hand, distinct from these, are the equally obvious, conscious 'products' of those acts—*what* is apprehended, *what* relation comes to be discerned, *what* is recalled, etc. On the one hand, too, stand the conscious *acts* of attending, deliberating, deciding; on the other hand stands the conscious 'material' *to which* we attend, *about which* we deliberate, *on which* we decide.

We should further agree, I thought, that all conscious *acts* are acts of what we term the 'self'—in other words, that all conscious activity invariably implies self-activity, and that, without some kind of 'self' (however rudimentary) which might receive or act on the results of other mental activity within the organism, there can be no consciousness either of act or of product of mental activity.

It is evident, however, that not all conscious 'material' or contents appear to be the products of *conscious* activity: for example, we are not bound to be consciously thinking in order that an idea should occur to us, and we receive sensations from stimuli and perceptions of familiar objects without being consciously active of the processes of sensing and perceiving. Hence arose the notion of conscious 'presentations' *to* the consciously active self; and hence, too, we may, at first sight at all events, conclude that the *contents* of consciousness may occur under one of two alternative conditions, namely either as the result of, or in the absence of, *acts* of consciousness.

At first sight, too, we may be tempted to conclude that, just as we may apparently have contents of consciousness without the necessity of having acts of consciousness, so we may have acts of consciousness without the necessity of having contents of consciousness. For we seem to be able to experience *an act* of apprehending, recalling or attending without necessarily experiencing *what* is apprehended, recalled or attended to. But I believe that this isolation of act from content is only apparent; it seems to me that we cannot actively attend, apprehend or endeavour to recall without attending to, apprehending or recalling something, however vague, indefinite, or irrelevant; a conscious act must yield a conscious product, even if it fail to take the form of a clear-cut and relevant percept, idea or relation.

But if 'acts' of consciousness are invariably associated with 'objects' of consciousness, may not conscious 'objects' or 'presentations'—*e.g.* the sudden sound of which we are forced to become aware or the idea which occurs to the mind—also really necessitate 'acts' of consciousness? We may admit that acts of sensation, acts of perception and acts of ideation may occur without involving conscious self-activity. But if, as I have premised, we are agreed that isolated fragments of consciousness are impossible, and that all normal consciousness is the resultant of one integrated and conscious self-activity, may it not be that the conscious products of unconscious mental activity (sensory, perceptual, ideational, etc.) become conscious by the act of being incorporated within that unitary mental system which we call the self, and that self-activity is essential for such incorporation? A sudden sound or an occurring idea would, on this view, remain unconscious until the self's activity—its act of attention (whether 'active' or 'passive')—had *ipso facto* rendered it conscious. And a similar attitude is applicable to the so-called unconscious wishes and unconscious feelings; we are powerless to describe them in terms of consciousness, *i.e.* as wishes or feelings, until they have become conscious.

We might on these lines be disposed to erect vast systems of *unconscious*, 'lower-level,' mental activities engaged in building up *unconscious*, 'lower level,' mental products which, in their final state of elaboration, may be presented to the mental system known as the 'self' and may, in virtue of the self's activity on them, ultimately become *conscious* contents or products. We cannot possibly deny the occurrence of important, congenitally and fundamentally unconscious, directive mental activities, such as occur, for example, in the integrative processes of association, in the improvements which occur during rest after practice, in the processes of perseveration and forgetting, in the creative acts of genius, and in the processes of intuition, suggestion, volition, conflict and recall. But here the fundamental question arises—is the self something primordially separate and distinct from these lower-level activities? Because we have termed them 'lower-level,' must they necessarily be *wholly spatially* at a lower level? In other words, does the self, in the course of mental evolution, owe its origin to the sudden emergence of totally new higher-level processes and its enrichment to the increasing entry and the complex integrations within the self of earlier and originally independent lower-level unconscious activities?

Is this a likely view? Or is it not more probable that the self is as old as the individual organism and that it has developed by releasing, by dissociating, consciousness from what now appear to us as lower-level unconscious mental activities? Is not the self rather primarily a diffuse, widely spread formation, and are not these so-called 'lower-level' mental activities secondary, often functional, rather than primary, spatial, formations? Regard, for example, the acquisition of habits within the life of the individual. Do they involve the cessation of self-activity and the relegation of mental activity to spatially lower, unconscious, levels? Is there a mental, and correspondingly a neural, system of the self, reigning, as it were, pontifically aloft, which functions only in the conscious learning of a new habit, ceasing to function any longer when that habit has been acquired and has become automatic? I venture to assert that we have no evidence, psychological or neurological, that different spatial 'levels' are involved according as a mental process is conscious or as, by repetition and familiarity, it becomes unconscious. I feel convinced that, whether or not an act has become habitual, whether or not in the performance of that act conscious self-activity occurs, the self is nevertheless in either case, but in different ways, active. The presence or absence of consciousness in self-activity must depend (*a*) biologically on the novelty of the situation and the need for adjustment, adaptation and direction of the whole organism, and (*b*) neurologically on the degree of resistance to, or of easy flow of, the resulting response in satisfying and restoring those disturbances from equilibrium to which the whole living organism is incessantly subjected. In other words, the presence or absence of consciousness depends on the kind of self-activity involved—on the biological need for *directive* activity of the entire self, in *addition* to its mechanical activity.

I do not mean wholly to deny the occurrence of ontogenetic and phylogenetic integrations and syntheses from original parts. But far more important, so it seems to me, is the differentiation, the analysis, of original wholes. Wherever we meet with a living individual organism, there we have, in however rudimentary a form, a conscious self, with at least some powers of adjustment, creation and foresight, engaged (may I here be permitted to quote from another address) in "selecting from alternative responses and from alternative environments those which are most advantageous to the ever evolving and developing organism. To secure the most suitable movements and environment and thus to help in the evolution of the organism are the prime objects of consciousness; and where, as in plants, mobility and plasticity are at a minimum, self-activity and consciousness are inappreciable[1]." If my view is correct, self-activity is to be regarded as the primary, fundamental, general directive activity, and not as the 'highest' unitary integration of 'lower,' originally independent, unconscious directive activities of the organism.

If this be the truer view, a vague diffuse glow of consciousness throughout the most primitive living organism has given place, in the phylogenetic evolution of mental life, to a more clearly defined, more restricted and more exalted, consciousness. That is to say, in the evolution of more complex living forms, lower-level, congenitally unconscious, mental activities have arisen by differentiation from what originally constituted activities of the conscious primitive self. According to this view, our humblest, purest, reflex action (which is unconscious both as regards act and product) is not the most primitive, but (in a sense) the most highly differentiated form of psycho-neural activity. And presentations to the self are to be regarded as the final outcome of the further evolution of higher self-activity—an evolution involving the differentiation of congenital lower-level activities never wholly distinct from the parent self-activity itself, which has, as it were, become 'distilled' from them.

Just as an acquired habit implies no entire absence of the self-activity which originated it, so sensation and perception in response to familiar stimuli or objects imply no entire absence of original self-activity (which is experienced indeed whenever a novel sensation or a novel percept occurs). Lower 'sensory' or even 'sensori-psychic' mechanisms and so-called 'centres' of corresponding neural activity there may well be; but they are not the centres (or seats) of *consciousness*, and they only function normally when they stand in normal relations with the entire organism on which the normal self surely depends. We have no right, then, to speak of independent, lower-level centres of conscious sensory or perceptual activity. Under pathological and experimental conditions these centres may reveal independent activity; but, like the behaviour of the spinal or decerebrate animal, their activity under these conditions is not identical with their activity in the intact organism. The same is to be said of those unconscious mental directive activities to which I have already alluded.

As we descend the scale of sensations, from visual and auditory experience, derived from *extra*-somatic stimuli, to kinaesthetic, thermal, algaesthetic and visceral experience, derived often largely or wholly from *intra*-somatic stimuli, we are able to trace a gradual change from *presentations to* the self (and their frequent projection as external 'objects') to *feelings* (or modifications) *of* the self. Clearly this affective mode is a far more primitive kind of consciousness than is the mode of contents of consciousness. Primordially, even colour must surely have been experienced as a mere 'colouring' *of* the self—an affective modification *of* the self, inducing and produced by self-activity. Primordially, then, there were no presentations *to* the self, but merely modifications *of* the self.

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[1] "On the Nature of Mind.' The Presidential Address of the author to the Section of Psychology at the Centenary Meeting of the British Association for the Advancement of Science, London, 1931.

Such must have been largely the conditions of conscious experience before the evolution of the cerebral cortex, during (and before) the evolution of the optic thalamus which is still primarily concerned with the affective, primitively sensitive and instinctive forms of mental life. We recognize, with the appearance of the cortex, the development of higher intelligence, fuller discrimination, and the finer discernment of relations and meanings. But even in pre-cortical times, all these must, according to my view, have been rudimentarily present. Intelligence has not suddenly 'emerged' at a certain stage of mental evolution, superimposed on purely instinctive reactions. Experimental animal psychology has clearly demonstrated that meanings and relations (and all meanings depend on relations) are fundamentally inherent in all conscious experience, however rudimentary. All mind, all life, involves relativity; the absolute (like the absolutely new) is a *terra incognita*. Weber's law is perhaps the most fundamental illustration of the importance of 'context' for consciousness; the phenomena of contrast and adaptation constitute another. All consciousness and all response depend not only on the nature of the 'stimulus' but also on their relation both to past conditions and to the totality of present conditions.

Even in the primordial modifications or colourings of the self we may discern the rudimentary appearance of 'meanings.' For why should the self be differently modified or consciously affected by, say, a blue or a red light (or by a high- and a low-pitched sound), unless such difference in consciousness was *serviceable* in evoking different, conscious, self-determined reactions—one colour (or pitch) evoking one biologically useful group of frequently alternative reactions, another colour (or pitch) another biologically useful group of reactions? Is it not clear that one kind of stimulus would come to 'mean' one kind of reaction, another kind of stimulus another kind of reaction? For fundamentally, what is 'meaning' but the ultimate *use* of a particular excitation, the tendency to some particular kind of act resulting from it—in other words, what that excitation is related to, or associated with, what it 'stands for'—not, of course, absolutely, but relatively in regard to the total context?

Thus each affection of the self may be said to have acquired its special conscious 'colouring' through its relation to the special series of responses which is consciously evoked. The meanings which these affections acquire, as phylogenetically they evolve (in their natural and environmentally more complex forms) into primitive percepts and (in their analytic, more abstract forms) into 'pure' sensations, is something obviously different from the later and fuller meanings ontogenetically acquired by percepts and ideas. Nevertheless, like all meanings, they are based on *relations*; and even at early stages there occurs too the *consciousness* of relations between conscious experiences, first as feelings of the self and later conjoined with the developing 'material' of consciousness.

But if, originally, conscious experience consisted merely or mainly in acts of the conscious self and in preceding and consequent modifications of the conscious self, how are we to account for the awareness of a persistent self, *i.e.* self-consciousness? This must have depended on continuity of activity, on memory—persistence and revival—on integration and disintegration, and thus on the formation of a single organized system of past experience, an ever active unitary stream of conscious awareness of *what* is acting and of *what* is being modified by such activity. This formation is, moreover, related to the two well-known kinds of memory—the memory of the *unique* individual acts of the self, and the memory of the sum total of corresponding modifications of (and later of presentations to) the self—the memory of unique acts of learning and the composite memory of what is learnt.

We may with advantage, I think, once again approach our subject from a standpoint other than a merely spatial one. Psychologists have long recognized the difference between the 'actual' and the 'specious' present—between a present which is an ideal moment of time and a present which is really a unitary blend of far longer duration. Is not the basis of this difference also the basis of the difference between the conscious acts of the self and the conscious modifications of and presentations to the self? Is it not, too, the basis of the difference between the conscious acts of the self and the conscious awareness of the self? Surely the very occurrence of continuity of activity and of persistence and recall, and the integration that yields awareness of the present (which is really a specious present), enable self-activity, with its powers of disintegration or dissociation, to become aware of its immediately past acts, to become aware of a self, and to become aware of modifications of that conscious self. Thus the self not only knows, but knows that it knows; it not only acts, but may regard itself as acting, and thus 'depersonalize' or 'distance' its acting; and, when subject to more fundamental dissociation, it may exhibit all the remarkable and well-known phenomena of multiple and alternating personalities.

If this view be correct, and if the 'material'—the contents or products—of consciousness is a mere development of modifications, or affects, of consciousness, then the notion of contents or products is a misnomer. Ideas, percepts and sensations are not comparable to the contents of a filled living bag, nor are they comparable to the products of merely *mechanical* activity. They are of the very tissue of *directive* self-activity, and have appeared with the very appearance of self-awareness. They are an instance of that fundamental characteristic of continuous living process which consists in persistence and recall, in virtue of which the functioning of the self comprises not only its momentarily present conscious acts, but also the persistence and revival of immediately previous and long past conscious acts. These build up the awareness of a systematized, continuous self and the consciousness of modifications of that self by successive acts of the self. There can be little doubt that the contents of consciousness are of a more mechanical nature than the acts of consciousness; the latter consisting principally in directive activity, the former only inducing and being related to such self-activity. But it would take us too far afield to-day to discuss this difference or, again, the differences between the consciousness of imaged and imageless, or of verbal and wordless thought, or of cognized and felt relations.

Originally, as I have tried to explain, the conscious self was so widespread as to include nearly every, if not every, mental directive act of the organism. In the process of evolution, independent direction became possible in the activity of lower differentiated levels, while consciousness became more and more restricted and exalted so that its presence came to depend on the activity of more and more highly differentiated, delimited and integrated, yet wide, areas of nervous tissue. And it is on this unitary integrating and disintegrating system that the acts and contents (as well, of course, as the affective side) of consciousness depend. The momentary activities of the self in the actual present give rise to conscious acts; its immediately past activities in the specious present through their continuity of origin contribute, with more remote revivals, to the generation of conscious products and affects and of self-awareness. Conscious acts and conscious products are fundamentally not two unrelated modes of consciousness but are the expression of a common, ultimate, individual self-activity, distinguished, like all living activity, by evincing inherent direction, purpose, creation and struggle for existence, besides being subject to the blind, lifeless forces of abstract mechanical activity.

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A TEST OF THE THEORY OF TWO FACTORS.

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I. *Introduction* (p. 352).
II. *The viewpoint of experimental psychology* (p. 353).
III. *An example of the working of the theory* (p. 354).
IV. *Requirements for a test of the theory* (pp. 354–355).
V. *The tests and their application* (pp. 355–357).
VI. *Scoring and correlation calculation* (pp. 357–358).
VII. *Correlations* (pp. 358–360).
VIII. *The problem of further specificalities* (pp. 360–361).
IX. *Statistical evaluation of results* (pp. 361–364).
X. *A subsidiary test* (p. 365).
XI. *Criticisms met* (pp. 365–366).
References (p. 367).

I. INTRODUCTION.

IN a critical article by Prof. Karl Pearson (1) on the Theory of Two Factors it is suggested that "some 12 to 15 abilities...the abilities being settled by psychologists *a priori* to avoid 'overlaps,' are essential to a satisfactory test [of this theory], the observations to be made on a homogeneous population of several hundreds" (p. 261). The present paper describes a test of the kind that Prof. Pearson demands, using correlations for some 20 abilities. The research was organized by one of us (W. Brown) over a year ago, in connection with his re-testing of his own earlier correlational material by Prof. Spearman's 'tetrad criterion' (2), and he has made himself particularly responsible for the mathematical arguments and conclusions of the investigation, including the curve-fitting. But the mental tests were devised and applied by Dr W. Stephenson, and the correlation coefficients and tetrad differences were also calculated by him. Some preliminary remarks, from both psychological and mathematical viewpoints, are called for.

II. THE VIEWPOINT OF EXPERIMENTAL PSYCHOLOGY.

The psychologist does not select the 20 abilities on a narrow *a priori* basis. The abilities are found by a slow process of experimentation and test refining. Moreover, this experimentation is itself based upon the theory of two factors. The psychologist devises tests that, approximately, should fit a theoretical criterion (that of zero 'tetrads').[1]

But it betrays a misconception of the nature of a scientific theory to say that, thereby, the psychologist is working in a closed circle. On the contrary, he works like a physicist; he establishes the fact that under certain conditions the criterion is satisfied, and he determines the nature of these conditions. The core of the matter is that in this way, and generally, the theory of two factors *works* for psychology. When reasonable agreement is found between the criterion and correlational facts, the common factor can receive an acceptable psychological explanation. When the criterion and facts do not agree the psychologist makes a determination of 'overlap,' 'group factors,' or 'specificality[2].' Again (and this is the essential matter), it is found that either the specificalities have acceptable psychological explanations, the true influence of which had been neglected, or that a field of research is opened up for facts that are essentially new and unexpected.

Thus, what for mathematics is a failure of the criterion is for psychology a pointer to new psychological findings. The proof of the theory is much more than a purely mathematical matter. There is in the proof the foundation and development of a scientific experimental psychology; and, although we would be modest, to that extent it constitutes a 'Copernican revolution.' The mathematician, given the data, might prove the theory to acceptable limits—it is so proved, even for the exacting conditions laid down by Prof. Pearson, in the course of the present paper. But only the psychologist can now disprove the theory.

The mathematics and the psychology of the theory of two factors are now so developed that the mathematician, before he can test out some of his sub-theories, can scarcely proceed unless the experimental data are as precisely as possible of the kind that the psychologist daily tries to supply for the progress of his science. It seems that neither the mathematics nor the psychology can be absolutely rigorous, any more than is the case in physics; and neither can proceed without the other.

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[1] Other tests can be devised, likewise, that should *not* fit the criterion, as is the case when tests are too similar.

[2] The term 'specificality' covers both positive ('overlap,' or 'group factor') and subtractive or negative influences. It stands for any disturbance of the tetrad criterion.

III. AN EXAMPLE OF THE WORKING OF THE THEORY.

An illustration of the *modus operandi* of the theory of two factors can serve to amplify the above viewpoint and introduce some of the requirements of a test of the theory.

It was found by one of us(3) that verbal intelligence tests did not, whereas certain non-verbal tests did, agree satisfactorily with the tetrad criterion. A theory of an additional *v*-factor, a group factor, was tried out for the verbal tests, and with further researches the full force of the influence of reproduction and experience on these verbal tests was made apparent. It now appears that only when past achievement, and therefore the rôle of retentivity and reproduction, is rendered as simple as possible, as when primarily perceptual tests are used, does the criterion agree well with the facts. A rôle for past experience and reproductive processes can scarcely be denied for the verbal tests, and the theory of two factors *works* because it isolates in a *v*-factor the activities in which experience and reproduction are strongest and most to be expected. *Pari passu*, the theory works in so isolating a purer field of perceptual tests within which eductive processes can be considered to hold sway.

IV. REQUIREMENTS FOR A TEST OF THE THEORY.

The important requirement is the set of 20 or so non-overlapping abilities.

It has been shown(4) that non-verbal tests, that were primarily perceptual in form, supplied correlations agreeing with the theory of two factors for a population of 1000; and, as has been suggested above, the non-overlapping tests are most likely to be those of perceptual foundation. The work of Line(5) must be mentioned as a pioneer study with such a test. At the time of beginning the present research there were not available 20 sufficiently developed primarily perceptual tests. But some non-verbal tests can be used, with the knowledge that many serve as 'pure' *g*-tests, each with *g*-factor and factors specific to the test, the latter condition obtaining because of the narrow range of the experiential or reproductive influences in them. For this reason two or more non-verbal tests, themselves not perceptual in any critical way, might act as 'reference values' for the primarily perceptual tests. The common visual fundaments of the latter may thus be controlled for specificality, although it seems impossible to conceive of any test that is less critical for all but eductive processes than these latter. We had available, then, 11 primarily perceptual, and 5 non-verbal, tests.

Now any *one* verbal test could be used together with the above 16 tests, in spite of *v*-factor content, because the *v*-factor would then be specific relative to the set-up of tests. But, in spite of their *v*-factor content, there were compensating reasons that made it desirable to include more than one verbal test in our battery. Verbal tests lend themselves to a freshness of working that helps along a long day's testing; and they introduce high correlations with some of the perceptual and non-verbal tests. Of greater moment, there would seem to be no reason why a partialling device should not be used, whereby the *v*-factor is partialled out, leaving the intercorrelations amongst the verbal tests attributable to a *g*-factor, so that these partialled correlations might then be used as non-overlapping values, then comparable with the correlations amongst the perceptual and non-verbal tests themselves. Evidence could be sought in the present work for the applicability of such a partialling technique. Thus, we added 6 verbal tests to our battery, making 22 tests in all.

With these tests specially prepared for group application to children of the age upon which it was decided to work, we have only to guard against sources of specificality of the varieties described by Spearman (6), and data should ensue that are suitable for the mathematician's detailed examination. A test of the theory of two factors requires a correlation table showing steep hierarchy, so that some of the tests need not be highly intercorrelatable. It would seem that a population of 300 is a minimum for use in fine correlational work.

V. THE TESTS AND THEIR APPLICATION.

Table I names the various tests, in order of application to the testees. The following notes will help to give meaning to the table.

Three hundred boys were tested in groups of not more than 25 per group. The boys were of age 10 to 10½ years at the time of testing, and were all the boys of that age in the two or three elementary school classes usually accommodating that age. Boys in lower classes, or those who suffered obviously from physical or scholastic disabilities, were not tested. Each group of boys attempted all 22 tests on the one school day, in four testing periods of about an hour each, commencing at 9.30 a.m., and ending at 4 p.m. A standard procedure was followed of demonstrating at least six sample test-units on the school blackboard just before beginning each test, and then allowing a fore-practice period to the testees, using test-units printed on the covering page of the test proper. The time allowance for demonstration and fore-practice, and for the test proper, is shown in Table I for each test.

A number is given to each test in Table I: the letter '*p*' following the test number indicates that the test is primarily perceptual; whilst '*n*' and '*v*' indicate that the tests are non-verbal and verbal respectively.

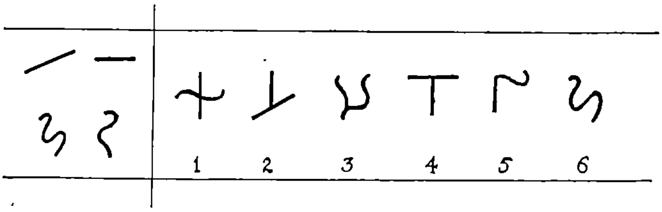

Fig. 1.

The tests numbered 1, 2, 3, 4, 5, 6, 10, 12, 13, 18 are described in previous papers (4); and in each case the test used in the present work was a development of that of the same name used in the work on 1000 children. Tests 10 and 11 are of the types developed by Fortes (7) and Line (5) respectively. No. 22 is the 'Pitch Perception' of Seashore's Musical Ability Test (8), applied by gramophone. No. 7 consisted of a set of mazes of the well-known Porteus kind, but here prepared specially for group application. Nos. 8, 14 and 20 are considered to be sufficiently known by name. It is not considered necessary to enter into further descriptive details about any of the tests mentioned above, so that only the tests numbered 15, 16, 17, 9 and 19 need be given a short description.

Fig. 1 shows a test-unit of the 'Abstraction' test (No. 15). The pair of drawings at the left-top are *alike* in a certain way; the second pair at the left-bottom are also *alike* in a certain way; and two of the six drawings numbered 1 to 6 have to be selected in which *both* the above-determined likenesses are to be found.

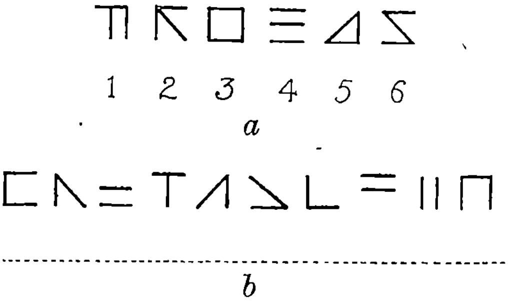

Fig. 2.

The Code test (No. 16) is a Substitution test. In No. 17 the same code items are used, but only *parts* of the code items are now provided, and the testee is required to substitute under each part the code number of the complete item. Fig. 2 (*a*) shows the code items used in test No. 16; and Fig. 2 (*b*) shows a short row of code-parts of test No. 17—the numbers 3, 2, 4, 1, 5, 6, ... etc., have to be substituted under these code-parts.

In test No. 9 (Pattern Perception) a pattern is given at the left, and this can be found exactly in the more complicated pattern at the right: a line has to be drawn around the determined pattern at the right. A test-unit is shown in Fig. 3.

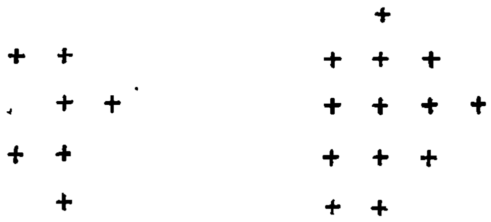

Fig. 3.

A sample test-unit of the Arithmetical Equations test, No. 19, is as follows:

$$3 \quad 9 \quad 5 = 7.$$

The signs + and − have to be placed between the numbers at the left side of the equation, so that the left side then equals the right side, *i.e.*

$$3 + 9 - 5 = 7.$$

VI. Scoring and correlation calculation.

The tests were scored by two competent psychologists, one test at a time for any one group of papers.

Because almost all the tests had been used previously in work on 10–11 year old groups, and were developed in accordance with results obtained for this age, the scatter of the score in each test was approximately '*normal*.' But, to obviate any disturbances in tetrads attributable to non-normal score distributions, a first step in the calculational work consisted in converting the crude scores for each test to values fitting a standard scale. The standard scale, used for every test, ranged from score 0 to score 19, with frequencies 1, 2, 4, 7, 10, 15, 21, 27, 30, 33, 33, ... and again downwards to score 1, for the scores 0 to 19 respectively[1].

Correlations were calculated, using the 'difference' method[2]. The 'differences' were squared and added, using Burrough's Adding Machine with tape recording. Seven-figure logarithms were used in all calculational work. But the available multiplication tables that were used later were for tetrad calculations for three figures only, so that it is sufficient to report correlations to three places of decimals only.

VII. CORRELATIONS.

Table II shows the first correlation table for 20 of the 22 tests. The tests numbered 21 and 22 are not used in the present work, because 20 is a sufficiently large number for our present purpose. The two, 21 and 22, were held in reserve; they were the tests last applied in the day, and this is the only reason why they, rather than any other of the 22 tests, are held in reserve.

But Table II cannot be employed as it stands for the test of the two factor theory. Six of the tests are verbal, entailing a *v*-factor, and two of the non-verbal, perceptual tests also entail specificality. There is required a procedure for partialling out such specificalities, and the following was adopted:

(i) First the *g*-factor is partialled out of the set of tests involving a known specificality, using perceptual tests as 'reference values.' Thus, taking the *v*-factor as a known specificality, the *g*-saturation of any verbal test is given by the following equation[3]:

$$r^2_{vg} = \frac{r_{vp_1} \cdot r_{vp_2} + r_{vp_1} \cdot r_{vp_3} + \cdots}{r_{p_1 p_2} + r_{p_1 p_3} + \cdots}.$$

Using *g*-saturation values obtained in this way, the *g*-factor is partialled out of the tests involving the known specificality by use of the following equation:

$$r_{v_1 v_2 \cdot g} = \frac{r_{v_1 v_2} - r_{v_1 g} \cdot r_{v_2 g}}{\sqrt{1 - r^2_{v_1 g}}\,\sqrt{1 - r^2_{v_2 g}}}.$$

(ii) Using the *g*-partialled correlations ('specific' correlations) given above, the specificality saturation is next determined. This repeats the first step of (i) above:

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[1] For a description of the converting method, see Stephenson (4).

[2] $$r = \frac{\Sigma X^2 + \Sigma Y^2 - \Sigma (X - Y)^2 - 2nm_x m_y}{2\sqrt{\Sigma X^2 - nm_x^2} . \sqrt{\Sigma Y^2 - nm_y^2}}.$$

[3] The subscripts '*v*' and '*p*' refer to 'verbal' and 'perceptual' tests respectively. The notation used in these first sections must not be confused with that of the Statistical Evaluation of Results, p. 361.

thus, the *v*-saturation of any verbal test is given in terms of the other verbal tests by the following equation:

$$r^2_{v_1 g \cdot \nu} = \frac{r_{v_1 v_2 \cdot g} \cdot r_{v_1 v_3 \cdot g} + \dots}{r_{v_2 v_3 \cdot g} + \dots}.$$

(iii) Now restart with the first correlations, and partial out the specificality, using the specificality-saturations given at (ii). Thus, to partial out the *v*-factor use the *v*-saturations determined as at (ii) in the following equations:

$$r_{v_1 v_2 \cdot \nu} = \frac{r_{v_1 v_2} - r_{v_1 g \cdot \nu} \cdot r_{v_2 g \cdot \nu}}{\sqrt{1 - r^2_{v_1 g \cdot \nu}} \sqrt{1 - r^2_{v_2 g \cdot \nu}}}.$$

The step (ii) can only be taken when the specificality correlations given at (i) themselves fit the tetrad criterion: there must therefore be at least four tests involving the same specificality, although three tests serve to determine specificality saturations. In most cases, however (other than for well-defined group factors like the *v*-factor), specificality is observed for *two* tests only; but to a first approximation the square root of the specific correlation can be taken to be the specificality-saturation of each of the two tests. Using this saturation value, the specificality can be partialled out as at step (iii) above.

The above partialling procedures are theoretically warranted in our work, and we do not offer any further substantiation of them, but it should be observed that the probable errors of the above partial correlations are not known, although they can be considered to be a little greater than those for the first correlations.

Apart from the *v*-factor there is obvious specificality in two correlations only in Table II, for the two Code tests (Nos. 16 and 17), and for two perceptual tests (Nos. 5 and 9). The Code tests are, of course, too similar to be free from overlapping. Using the above partialling procedure we can partial out Code-specificality, leaving a correlation $r_{16 \cdot 17}$ attributable to *g*-factor only. In this way the first correlation 0·644 is reduced to 0·452. As for the tests 5 and 9, it was the first time that both had been used together in a correlation table, and the tests are apparently too similar in some way, the spatial fundaments are similar and both require a certain drawing ability. Both have fairly easy test-units, and thus 'speed-preference' is not an unlikely disturbance. But the correlation $r_{5 \cdot 9}$ need not concern us further; we shall find later that the test No. 5 is the source of other disturbances. The above partialling procedure results in a value 0·518 in place of 0·655 for $r_{5 \cdot 9}$, the value 0·518 now being attributable to *g*-factor only.

A correlation table 'corrected' for *v*-factor, Code-specificality, and specificality between the two similar perceptual tests, is given as

Table III, the correlations now being reported in hierarchical order. They are now ready for tetrad examination. But, again, only after the tetrad examination can we decide whether the correlations are acceptable as 'non-overlapping' values.

VIII. THE PROBLEM OF FURTHER SPECIFICALITIES.

There are 14,535 tetrad differences for a table of 20 tests. These were set down by Stephenson, and products and differences were computed by a well-trained and capable worker, using Cotsworth's 'Direct Calculator.'

Only some 23 of the 14,535 tetrad differences have values greater than 0·1000. An examination of the tetrads shows that the correlation $r_{6\cdot 18}$ (for the two verbal tests, 'Understanding Paragraphs' and 'Classification') accounts for seven of these large values, and that the mean of all tetrads involving this correlation is more than five times probable error[1]. This is a partial correlation, and its probable error is likely to be larger than that for a first correlation: but, even so, the two tests are at the opposite extremes of quality-quantity preference—the 'Classification' is a *speed* test of a marked kind (only four words have to be regarded in each test unit, one of which is *unlike* the other three), whilst the 'Understanding Paragraphs' is much more a *power* test (involving the regard of a long paragraph, followed by answering questions about the paragraph). 'Speed preference' gives an advantage to the former, and a disadvantage to the latter test, and a misbalance of this kind frequently leads to specificality for the two tests concerned. It thus seems allowable to discard tetrads which involve this correlation $r_{6\cdot 18}$. (We could, if need be, re-score the two tests, penalizing for 'speed' in the one case, and allowing extra marks for it in the other, and so remove the disturbance.)

Finally, one test, that of 'Fitting Shapes,' can be considered to account for nearly all other tetrad differences greater than 0·1000, the correlations of this test with tests numbered 2, 7, 9, 10 and 17 being particularly concerned. It is of interest that these tests amongst themselves appear to show some slight signs of a group factor, on the borderline of significance. All these tests involve 'spatiality,' and either this or speed preference could account for a group factor. If test No. 5 ('Fitting Shapes') was the most highly saturated of a set of tests involving such a group factor, then the data we have obtained are explicable. It would seem acceptable to omit the test No. 5 from tetrad consideration. At worst it is no sin to omit *one* test from a battery of so many, even were there no readily acceptable explanation to be offered to the specificality that it entails.

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[1] Using an approximate probable error, given by C. Spearman, *Abilities of Man*, Appendix, p. x, formula 14.

After removing the test No. 5, and all tetrads involving the correlation $r_{6\cdot 18}$ (and these only), there remain 11,356 positive tetrad differences, with frequency distribution given in Fig. 4. This is the first set of tetrads that are offered as those obtained for non-overlapping tests and that can receive the statistician's attention.

IX. STATISTICAL EVALUATION OF RESULTS.

We have first to determine the constants of the distribution of the 170 correlation coefficients. Grouping in intervals of 0·05, we have:

$\bar{r} = 0{\cdot}41353$ (0·41367 by actual averaging of separate r's);

$\sigma_r = \sqrt{\mu_2} = 0{\cdot}087268$;

$\mu_2 = 0{\cdot}0076157$; $\mu_3 = -0{\cdot}00016834$; $\mu_4 = 0{\cdot}00015647$;

$\beta_1 = 0{\cdot}064157$; $\beta_2 = 2{\cdot}6978$; $\therefore\ \beta_4 = 11$ (approx.)[1].

(These are the values after Sheppards' corrections had been applied. The uncorrected values were:

$\sigma_r = 0{\cdot}088398$; $\mu_2 = 0{\cdot}007824$; $\mu_3 = -0{\cdot}00016834$;

$\mu_4 = 0{\cdot}00016623$; $\beta_1 = 0{\cdot}05917$; $\beta_2 = 2{\cdot}7219$.)

Accordingly from the Spearman-Holzinger formula (9)

$$\bar{\sigma}_t^2 = \frac{4}{N}\left\{\bar{r}^2(1-\bar{r})^2 + \left[1 - 3\bar{r}\frac{(n-4)}{(n-2)} + 2\bar{r}^2\frac{(n-6)}{(n-2)}\right]\sigma_r^2\right\},$$

where N = no. of cases (300) and n = no. of tests (19)

$= 0{\cdot}00079937$,

$\bar{\sigma}_t = 0{\cdot}02827$.

[Taking $\bar{r} = 0{\cdot}41367$, $\bar{\sigma}_t = 0{\cdot}0282612$.]

In order to be able to compare this 'theoretical' value with our 'observed' result (to be given later) we must determine its 'probable error.' For this purpose we have employed the following formulae, given in the article of Karl Pearson and Margaret Moul (1)[1]:

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[1] *Tables for Statisticians and Biometricians*, vol. I, Table XLII (*b*), p. 78.

$$\sigma^2_{\bar{r}} = \frac{1}{Np}\left[(1-\bar{r}^2)^2 - 2\mu_2(1-3\bar{r}^2) + 4\bar{r}\mu_3 + \mu_4 + (n-2)\,\bar{r}\,(1-\bar{r})^2\,(2+n\bar{r})\right]$$

$$= 0{\cdot}0468664 \ (p = \text{no. of correlation coefficients} = 170),$$

$$\sigma^2_{\mu_2} = \frac{4}{Np}\left[(1-\bar{r}^2)^2\,\mu_2 - 4\bar{r}\,(1-\bar{r}^2)\,\mu_3 + 2\,(1+\bar{r}^2)\,\mu_4 - 4\bar{r}\beta_4\mu_2^3\right]$$

$$(\beta_4 = 11, \text{ approx.})$$

$$\doteq 0{\cdot}00000044839;$$

$$\{\delta\bar{r}\delta\mu_2\} = \frac{2}{Np}\left[-4\bar{r}\,(1-\bar{r}^2)\,\mu_2 - 2\,(1-3\bar{r}^2)\,\mu_3 + 4\bar{r}\mu_4\right]$$

$$= -\,0{\cdot}00000039295;$$

$$\sigma^2_{\sigma_t^2} \doteq \frac{64}{N^2}\left(\bar{r}\,(1-\bar{r})\,(1-2\bar{r}) + \frac{3\,(1-2\bar{r})}{p-1}\,\mu_2\right)^2\sigma^2_{\bar{r}}$$

$$+ \frac{16}{N^2}\left(1 + \frac{6\bar{r}\,(1-\bar{r})}{p-1}\right)^2\sigma^2_{\mu_2}$$

$$+ \frac{64}{N^2}\left(\bar{r}\,(1-\bar{r})\,(1-2\bar{r}) + \frac{3\,(1-2\bar{r})}{p-1}\,\mu_2\right)\left(1 + \frac{6\bar{r}\,(1-\bar{r})}{p-1}\right)\{\delta\bar{r}\delta\mu_2\}$$

$$= 0{\cdot}000000057591106;$$

$\therefore\ \sigma_{\sigma_t^2} = 0{\cdot}00023998,$

$\sigma_{\sigma_t} = \sigma_{\sigma_t^2}/2\sigma_t$, assuming approx. normal distribution ((1), p. 268)

$= 0{\cdot}0038349;$

$\therefore$ P.E. of $\bar{\sigma}_t = \mathit{0{\cdot}0025866}$.

Thus the 'theoretical' value, $\bar{\sigma}_t = \mathit{0{\cdot}02827} \pm \mathit{0{\cdot}0025866}$.

Turning now to the observed frequency distribution of tetrads, grouped in subranges of 0·005, we have the following distribution-constants:

Median (Quartile of Symmetrical Distribution) = 0·0214,

$\sigma_t = 0{\cdot}031289, \qquad \mu_2 = \sigma_t^2 = 0{\cdot}000979, \qquad \mu_4 = 0{\cdot}00000269749;$

$$\therefore\ \beta_2 = \frac{\mu_4}{\mu_2^2} = 2{\cdot}81446 \text{ (a platykurtic curve).}$$

As the tetrad-differences must lie numerically between -1 and $+1$, the best-fitting curve must be a limited range symmetrical curve. Since $\beta_2 < 3$, the best-fitting curve is a Type II_a Pearson Curve[1], with equation

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[1] Pp. 258, 259, 260. We have written p = no. of correlation coefficients, and n = no. of tests, reversing Pearson's notation, for the sake of uniformity with the Spearman-Holzinger formula.

$$y = y_0\left(1 - \frac{x^2}{a^2}\right)^m.$$

Here $m = \dfrac{5\beta_2 - 9}{2(3 - \beta_2)} = 13{\cdot}669$,

$a^2 = \dfrac{2\mu_2\beta_2}{3 - \beta_2} = 1188$ [$\mu_2 = 39{\cdot}16$ in terms of unit of grouping $(0{\cdot}005)$];

$\therefore\ a = 34{\cdot}468$ (or $0{\cdot}17234$ in terms of size of tetrads),

$$y_0 = \frac{N \times \Gamma(2m+2)}{a2^{2m+1}\{\Gamma(m+1)\}^2} \qquad (N = 22{,}712)$$
$$= 1412.$$

Hence the equation to the best-fitting curve is:

$$y = 1412\left(1 - \frac{x^2}{1188}\right)^{13{\cdot}669} \quad \text{(unit of grouping} = 0{\cdot}005\text{)}.$$

Or, expressed in terms of tetrad-differences,

$$y = 1412\left(1 - \frac{x^2}{0{\cdot}17234^2}\right)^{13{\cdot}669},$$

showing that it cuts the axis of x at the points $\pm\ 0{\cdot}17234$, making very 'high' contact.

The best-fitting normal curve

$$y = \frac{N}{\sigma\sqrt{2\pi}}e^{-\frac{x^2}{2\sigma^2}}$$

gives the equation $y = 1448e^{-\frac{x^2}{78{\cdot}32}}$ (unit of grouping $= 0{\cdot}005$).

The accompanying figure (Fig. 4) gives the symmetrical distribution of tetrad-differences, with the Type II_a curve (continuous line) and normal curve (dotted line) superposed.

Applying the (P, χ^2) test for goodness of fit[2], we have for the Type II_a curve, (mid-ordinates),

$$\chi^2 = S\left\{\frac{(m_r - m'_r)^2}{m_r}\right\} = 21{\cdot}69494$$

for 22 groups in half the symmetrical curve;

$$\therefore\ P = 0{\cdot}41764,$$

---

[1] *Tables for Statisticians and Biometricians*, Part I, p. lxiii.

[2] As Prof. Karl Pearson points out ((1), p. 276) this test is not wholly suitable to tetrad-differences, since "it is based on random selection from an infinite population, any member of which is equally likely to be drawn," but it is useful in giving us at least comparative values. For the Tables, see *Tables for Statisticians and Biometricians*, Part I, Table XII, p. 28.

—a good fit, since one sample in 2·4 would give a fit as bad or worse to this curve.

For the normal curve, (areas), $\chi^2 = 46{\cdot}82078$,

$$\therefore\ P = 0{\cdot}002612,$$

—far less good a fit, although if we look at the visual pictures of the two curves in the diagram we see that the normal curve does not fall greatly behind the Type II_a curve in closeness of correspondence to the observed values of the tetrad-differences. It is because of the large number of groups (22 for each half of the curve) that the numerical test of goodness of fit is so stringent and exacting in this case.

The above graduations were carried out in terms of mid-ordinates in order to be able to plot the curves. For the normal curve, areas were also calculated. In the case of the Type II_a curve, Simpson's quadrature formula, viz.,

$$\int_0^1 y\,dx = \frac{1}{6}(y_0 + 4y_{\frac{1}{2}} + y_1),$$

to change ordinate values into areas, was tried, but it made a difference of only 1·5 in the two largest groups, and less than 1·0 in all the other groups. Hence for our present purpose the correction was unnecessary[1]. Sheppards' corrections, for high contact, had already been applied in determining the moments of the distribution.

If m be taken as 14, the nearest whole number to 13·669, the equation to the curve becomes

$$y = 1428\left(1 - \frac{x^2}{0{\cdot}17234^2}\right)^{14}.$$

For this curve $\chi^2 = 31{\cdot}71508$, therefore $P = 0{\cdot}07749$—a less good fit.

The one constant of our observed frequency-distribution which we can compare with the previously determined 'theoretical' value is

$$\sigma_t = 0{\cdot}031289.$$

The excess of this over the theoretical value (0·02827) is 0·003019, *i.e.* 1·167 (or 1⅙) times the 'probable error' of the latter value (0·0025866). This means that the odds are about 4 to 3 against such a deviation—a good correspondence of observation with theory.

We may therefore conclude that, so far as this one frequency-constant is concerned, the criterion in the Theory of Two Factors satisfactorily passes the test of experience.

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[1] The high value of χ^2 for the normal curve is mainly due to the 'tail' of the distribution, viz., 9 for the 22nd group 'over 0·105,' although this represents no more than 0·8 per 1,000 distribution. Neglecting this tail, we find χ^2 for 21 groups $= 37{\cdot}82078$, giving $P = 0{\cdot}01913$.

X. A SUBSIDIARY TEST.

The frequency-distribution of tetrads for 15 of the tests, omitting tests *a*, *b*, *c*, *d* and *j* of Table III, was worked out, using the same unit of grouping, 0·005. For these 4095 positive tetrads,

$$\sigma_t = 0{\cdot}03116,$$

a result no better than that for 19 tests.

The other constants were:

$$\mu_2 = 0{\cdot}000971095, \quad \mu_4 = 0{\cdot}000002486, \quad \beta_2 = 2{\cdot}6362,$$

giving a markedly platykurtic Type II_a curve, with equation,

$$y = 497\left(1 - \frac{x^2}{0{\cdot}1186^2}\right)^{5{\cdot}746},$$

cutting the axis of x at the tetrad values ± 0·1186.

XI. CRITICISMS MET.

It has been pointed out by Prof. Karl Pearson ((1), p. 247), that "if ρ_{st} represents the correlation between the *s*th and *t*th variates in an indefinitely large population which is going to be sampled, r_{st} the corresponding correlation in any particular sample, and $\bar{r}_{st}$ is the mean value of r_{st} for many samples; then $\bar{r}_{st}$ is *not* equal to ρ_{st}." Assuming normal distribution of the variates, an approximate expression of the relationship (10) is

$$\bar{r}_{st} = \rho_{st}\left[1 - \frac{1-\rho^2_{st}}{2N} + (\ldots)\frac{1}{N^2} + \ldots\right],$$

where N is the size of the sample.

Moreover "the variation of r round ρ due to random sampling is of the order of its standard deviation, namely $1/\sqrt{N}$." Hence in writing r in the place of ρ, as we must since we do not know ρ, and as Spearman and Holzinger do in deducing their formula, terms of the order $1/N\sqrt{N}$ are neglected. If N is small this neglect may be serious. By applying our tests to 300 cases we escape the main force of this criticism.

A second criticism of a mathematical nature is that of Prof. E. B. Wilson (11) and Prof. H. T. H. Piaggio (12), that g is not determinate. But it has been shown by Prof. Spearman (13) that this indeterminacy diminishes as the number of tests is increased, and Dr J. O. Irwin (14) has proved "that g is not determinate but that its determinacy can be made as small as we please by taking a sufficient number of tests." We may fairly assume that the 19 tests employed in the present research are a large enough number to satisfy this condition.

We are still faced with the further objection of Prof. Wilson that "this uniqueness (of g) is relative to the set-up, since, if we constructed artificial tests by taking linear combinations of the marks in the original tests in such a manner as to preserve the hierarchical conditions, the new g would not necessarily be the same as the old" (quoted from J. O. Irwin (14), p. 363). Nevertheless we need not as psychologists be greatly disturbed by this objection.

The relation between the tetrad criterion and the "correlation between columns" (hierarchical order of correlation coefficients) criterion has recently been reconsidered in an interesting paper by Dr J. C. Maxwell Garnett (15). One of the outcomes of his discussion seems to be the propounding of a quantity y which he conceives to be related to g as follows: "g measures how much the individual tries throughout the set of tests, y measures how much good his trying does" (15) (p. 372). But he admits that this is "only a rough guess and probably a wrong one." The equation connecting the two quantities is:

$$g = G + ky,$$

where G is the most probable value of g for the given mental tests of the same individual, and "k is to be determined so that the standard deviation of y is unity." The argument is directly linked up with the work of Wilson and Piaggio. But since k tends towards zero as the number of tests is increased (Spearman and Irwin), the interpretation of y is not without difficulty. Dr Maxwell Garnett writes: "...there seems no reason why y should not be *the same* for the same individual *in every set of mental tests*. If, for example, y measured the individual's 'power of concentration'—perhaps how much 'mental energy' he renders available by a unit 'effort of will'—y might be independent of his performances $q_1, \ldots, q_n$ in any particular set of tests. Then g might measure how much 'mental energy' he manifests in the set of tests, so that g would depend on y and would be common to all the tests of the set" (p. 371).

We hope that further analysis of our data may throw light on this and cognate problems, but the work must be postponed to a subsequent article. The main purpose of our research has been achieved, namely to establish Prof. Spearman's Theory of Two Factors on an adequate statistical basis. We have also found that a Type II$_a$ Pearson Curve fits the distribution of tetrads for our 19 tests very closely.

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Table I. *Showing the tests in order of application.*

Testing period	No.	Type	Test name	Time allowed for demonstration	Time allowed for the test proper
1st	1	*v*	Inventive synonyms	2 min.	3 min.
	2	*n*	Alphabetical form	5	4
	3	*n*	Alphabetical series	3	8
	4	*v*	Disarranged sentences	2	6
	5	*p*	Fitting shapes	3	8
	6	*v*	Understanding paragraphs	1	12
2nd	7	*p*	Mazes	2	4
	8	*n*	Cancellation	3	3
	9	*p*	Pattern perception	4	5
	10	*p*	Analogies form	5	6
	11	*p*	Classification, 'rights'	6	12
3rd	12	*n*	Mutilated pictures	2	4
	13	*p*	Overlapping shapes	6	6
	14	*v*	Inferences, selective	2	6
	15	*p*	Abstraction, 'pairs'	4	8
	16	*p*	Code	2	3
	17	*p*	Code parts	2	3½
4th	18	*v*	Classification, selective	2	4
	19	*n*	Arithmetical equations	4	6
	20	*v*	Proverbs, selective	2	10
	21	*p*	Series form	5	8
	22	*p*	Pitch perception	5	10

Table II. *Showing first correlations for* 20 *tests, population of* 300 *boys, age* 10–10½ *years.*

(The numbers at the extreme left of the Table refer to the corresponding numbers in Table I.)

	Inventive synonyms	Disarranged sentences	Understanding paragraphs	Inferences	Classification (verbal)	Proverbs	Alphabetical form	Alphabetical series	Cancellation	Mutilated pictures	Arithmetical equations	Fitting shapes	Mazes	Pattern perception	Analogies	Classification 'rights'	Overlapping shapes	Abstraction 'pairs'	Code	Code-parts
1. Inventive synonyms	—	656	764	589	657	577	451	494	274	345	489	373	305	429	374	448	373	408	352	403
4. Disarranged sentences	.	—	702	566	659	500	442	602	344	396	584	465	324	516	507	481	467	499	436	496
6. Understanding paragraphs	.	.	—	644	645	620	399	571	363	358	585	453	372	525	476	520	465	507	456	488
14. Inferences	.	.	.	—	625	548	385	546	250	300	521	374	296	426	454	486	419	435	415	469
18. Classification (verbal)	.	.	.	.	—	521	420	526	248	285	532	404	268	414	447	449	389	409	388	440
20. Proverbs	.	.	.	.	.	—	327	418	174	246	446	315	203	340	421	417	315	386	324	388
2. Alphabetical form	.	.	.	.	.	.	—	515	293	277	501	476	300	411	417	416	398	359	304	474
3. Alphabetical series	.	.	.	.	.	.	.	—	332	322	549	570	407	534	575	593	508	477	440	491
8. Cancellation	.	.	.	.	.	.	.	.	—	176	384	312	314	364	359	376	277	257	443	408
12. Mutilated pictures	.	.	.	.	.	.	.	.	.	—	350	394	334	401	398	309	266	289	245	375
19. Arithmetical equations	.	.	.	.	.	.	.	.	.	.	—	425	358	549	522	513	491	460	444	557
5. Fitting shapes	.	.	.	.	.	.	.	.	.	.	.	—	429	655	542	475	437	359	345	510
7. Mazes	.	.	.	.	.	.	.	.	.	.	.	.	—	404	407	318	267	285	322	442
9. Pattern perception	.	.	.	.	.	.	.	.	.	.	.	.	.	—	541	468	441	394	391	547
10. Analogies	.	.	.	.	.	.	.	.	.	.	.	.	.	.	—	506	441	463	397	465
11. Classification 'rights'	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	—	468	477	393	432
13. Overlapping shapes	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	—	435	459	436
15. Abstraction 'pairs'	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	—	453	482
16. Code	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	—	644
17. Code-parts	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	—

Table III. *Showing correlations for 20 tests in hierarchical order; v-factor and two specificalities partialled out.*

(The numbers at the extreme left refer to the same numbers in Tables I and II.)

		Alphabetical series	Arithmetical equations	Understanding paragraphs	Disarranged sentences	Code-parts	Analogies	Pattern perception	Classification 'rights'	Inferences	Fitting shapes	Classification (verbal)	Abstraction pairs	Inventive synonyms	Overlapping shapes	Alphabetical form	Code	Proverbs	Mazes	Mutilated pictures	Cancellation
		a	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*j*	*k*	*l*	*m*	*n*	*o*	*p*	*q*	*r*	*s*	*t*
3. Alphabetical series	*a*	—	549	571	602	491	575	534	593	546	570	526	477	494	508	515	440	418	407	322	332
19. Arithmetical equations	*b*	.	—	585	584	557	522	549	513	521	425	532	460	489	491	501	444	446	358	350	384
6. Understanding paragraphs	*c*	.	.	—	558	488	476	525	520	474	453	368	507	500	465	399	456	445	372	358	363
4. Disarranged sentences	*d*	.	.	.	—	496	507	516	481	418	465	510	499	481	467	442	436	333	324	396	344
17. Code-parts	*e*	.	.	.	.	—	465	547	432	469	510	440	482	403	436	474	452	388	442	375	408
10. Analogies	*f*	.	.	.	.	.	—	541	506	454	542	447	463	374	441	417	397	421	407	398	359
9. Pattern perception	*g*	.	.	.	.	.	.	—	468	426	518	414	394	429	441	411	391	340	404	401	364
11. Classification 'rights'	*h*	.	.	.	.	.	.	.	—	486	475	449	477	448	468	416	393	417	318	309	376
14. Inferences	*i*	.	.	.	.	.	.	.	.	—	374	467	435	385	419	385	415	405	296	300	250
5. Fitting shapes	*j*	.	.	.	.	.	.	.	.	.	—	404	359	373	437	462	345	315	429	394	312
18. Classification (verbal)	*k*	.	.	.	.	.	.	.	.	.	.	—	409	392	389	420	388	324	268	286	248
15. Abstraction 'pairs'	*l*	.	.	.	.	.	.	.	.	.	.	.	—	408	435	359	453	386	285	289	257
1. Inventive synonyms	*m*	.	.	.	.	.	.	.	.	.	.	.	.	—	373	416	352	374	305	345	274
13. Overlapping shapes	*n*	.	.	.	.	.	.	.	.	.	.	.	.	.	—	398	408	315	267	266	277
2. Alphabetical form	*o*	.	.	.	.	.	.	.	.	.	.	.	.	.	.	—	304	327	300	277	293
16. Code	*p*	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	—	324	322	245	343
20. Proverbs	*q*	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	—	203	246	173
7. Mazes	*r*	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	—	334	314
12. Mutilated pictures	*s*	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	—	176
8. Cancellation	*t*	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	—

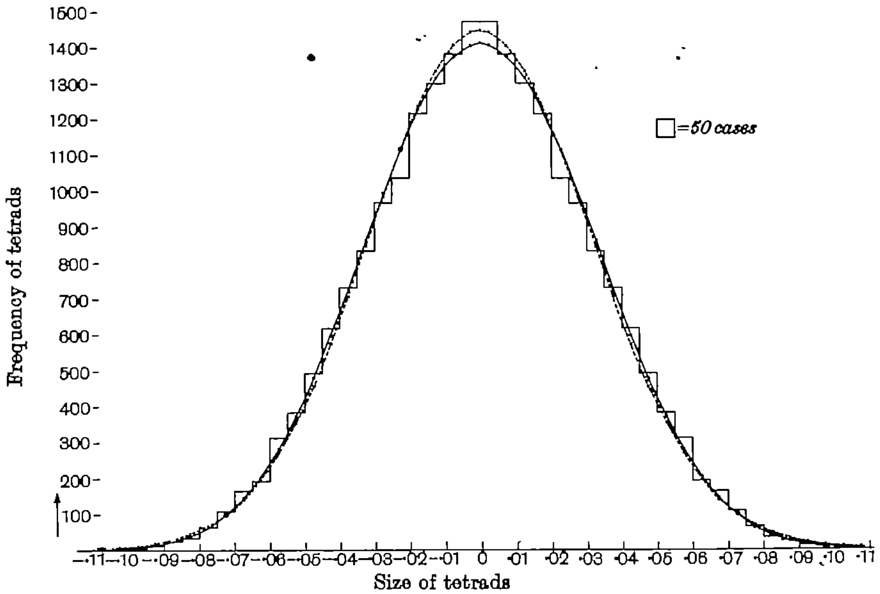

Fig. 4. Symmetrical distribution of the tetrad differences.

Best-fitting curve (Type II_a Pearson curve) ————————

Best-fitting probability curve – – – – – – – – – – – –

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Table IV. *Distribution of positive tetrads.*

Range of tetrad	Observed frequency	Type II_a curve (mid-ordinates) $y_0 = 1412$	Normal' curve (mid-ordinates) $y_0 = 1448$	'Normal' curve (areas)
0·000–0·005	1472	1408	1443	1441·8
0·005–0·010	1383	1376	1407	1405·6
0·010–0·015	1300	1314	1337	1335·8
0·015–0·020	1218	1225	1238	1237·0
0·020–0·025	1037	1116	1118	1117·9
0·025–0·030	968	992	984	984·7
0·030–0·035	832	861	844	843·3
0·035–0·040	731	728	706	706·4
0·040–0·045	618	599	576	576·0
0·045–0·050	493	480	457	458·0
0·050–0·055	383	373	354	355·0
0·055–0·060	311	281	268	268·2
0·060–0·065	193	205	197	197·6
0·065–0·070	163	145	141	141·8
0·070–0·075	109	99	99	99·3
0·075–0·080	65	64	67	67·7
0·080–0·085	33	40	45	45·0
0·085–0·090	24	24	29	29·2
0·090–0·095	11	14	18	18·5
0·095–0·100	8	7	11	11·4
0·100–0·105	4	4	7	6·8
Over 0·105	—	1	10	9·0
Total	11,356	11,356	11,356	11,356

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A CRITICAL DISCUSSION OF THE SINGLE-FACTOR THEORY[1].

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THE theory of the Single General Factor introduced by Prof. Spearman has been used extensively in the field of mental tests. The theory is based partly on mathematical and partly on psychological arguments, but in discussing it and in its application, the psychological and mathematical elements have perhaps not always been clearly distinguished. A mathematical statistician, such as myself, cannot hope to deal with the psychological implications, but by endeavouring to state as clearly as possible what the mathematical position is, what mathematical postulates in fact are required by the theory, he may help to clear the ground and leave the psychologist free to draw conclusions from these postulates together with the observed facts.

Let us briefly consider what the Single Factor Theory is. If we have examined a number of people in a series of tests a, b, c—and if the performance of any particular person (x) in these tests is represented by the marks m_{ax}, m_{bx}, m_{cx}, and if

$$\begin{aligned} m_{ax} &= \lambda_a g_x + \lambda_a' S_{ax}, \\ m_{bx} &= \lambda_b g_x + \lambda_b' S_{bx}, \\ &\ldots\ldots\ldots\ldots\ldots\ldots, \end{aligned}$$

where g_x is uncorrelated with S_{ax}, S_{bx}, S_{cx}, ..., and the S's are uncorrelated with one another; that is to say that if the marks in any particular test depend on two factors, one of which is the same for all tests and the other specific to the test in question, then it may be shown that if we work out the correlation coefficients between every pair of tests, every tetrad such as

$$r_{ab} r_{cd} - r_{ac} r_{bd}$$

will be equal to zero.

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[1] A paper read to Section J of the British Association, September, 1932. The term "*Single Factor Theory*" has been used in preference to "*Theory of Two Factors*" because only the nature of g is considered here; specific abilities are not dealt with.

Conversely, if the tetrads vanish it may be shown that there is a single general factor.

So much is incontestable, but at this stage we have to face three difficulties:

(1) In actual practice the correlation coefficients are subject to sampling errors, and we require to know how large our tetrad differences may be owing to fluctuations of sampling, when there is in fact a single general factor present. It is not proposed to discuss the sampling problem in this paper; it is sufficient to say that while existing approximations to the sampling errors are probably good enough to give us some appreciation of the significance or non-significance of a set of tetrad differences met with in practice, I think that a really satisfactory treatment of the sampling problem is still to come.

(2) It is not sufficient to be able to demonstrate the existence of a single general factor. We require to measure it. Here another difficulty comes in. Recent work of Heywood(4), of Piaggio(3), and of E. B. Wilson(1, 2) shows that the vanishing of the tetrad differences is not a sufficient criterion completely to determine g. There is an indeterminacy in the solution which may, however, be made as small as we please by increasing the number of tests used. (See also (5).)

In work of this kind it is usual to express the test scores in standard measure; that is to say, each person's score is expressed as a deviation from the average in terms of the standard deviation as a unit. If we do this and denote a person's score in a series of tests "*a*," "*b*," "*c*," ... by *a*, *b*, *c*, ..., then it may be shown that

$$g = \kappa^2 t + \kappa' i,$$

where

$$t = \frac{\mu_a . a}{{\mu_a}^2 - 1} + \frac{\mu_b . b}{{\mu_b}^2 - 1} + \dots,$$

$$\kappa^{-2} = 1 + \frac{1}{{\mu_a}^2 - 1} + \frac{1}{{\mu_b}^2 - 1} + \dots,$$

$$\mu_a = \left\{\frac{r_{bc}}{r_{ab} r_{ac}}\right\}^{\frac{1}{2}} = \frac{1}{r_{ag}}, \qquad \dots\dots\text{(i)}$$

with similar expressions for μ_b, μ_c, ...

Here everything can be estimated from the data except i, which is an arbitrary variable with unit standard deviation uncorrelated with any of the test scores.

In order to get an appreciation of the numerical importance of the indeterminacy represented by i, the following experiment was performed.

Five samples[1] of 25 were drawn at random from a universe in which the variate could vary from 0 to 100, the distribution in the universe being as nearly as possible normal. These five samples (headed A, B, C, D, G) are shown in Table I. G was taken as representing the single factor and the values

$$a=\frac{A+0{\cdot}5G}{1{\cdot}5},\quad b=\frac{B+0{\cdot}6G}{1{\cdot}6},\quad c=\frac{C+0{\cdot}7G}{1{\cdot}7},\quad d=\frac{D+0{\cdot}8G}{1{\cdot}8}$$

were calculated. a, b, c, d were then taken as the scores of 25 subjects in four tests, and the theory was applied to see how nearly it would reproduce G. In Table I a, b, c, d and G are also shown in standard measure.

We may calculate:

(α) The expected values of the correlations between a, b, c, d, on the assumption that A, B, C, D, G are all uncorrelated with one another. These values will satisfy the tetrad criterion exactly.

(β) The observed values of the correlations from our samples of 25. These will differ from the former because owing to fluctuations of sampling our 25 values of A, B, C, D, G will not be quite uncorrelated. Actually we find

	Expected values	Observed values
r_{ab}	0·23	0·43
r_{ac}	0·26	0·36
r_{ad}	0·28	0·21
r_{bc}	0·30	0·56
r_{bd}	0·32	0·51
r_{cd}	0·36	0·44

The observed values are well within their sampling errors in magnitude, but five out of the six are higher than the expected (the conditions of random sampling being in this respect not quite satisfied).

The observed values give us

$$r_{ab}r_{cd}=0{\cdot}19,$$
$$r_{ac}r_{bd}=0{\cdot}18,$$
$$r_{ad}r_{bc}=0{\cdot}11,$$

so that our three observed tetrad differences are 0·01, 0·07, 0·08.

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[1] The samples used were obtained by Dr Shewhart of the Bell Telephone Company. 4000 drawings of a chip from a bowl were made with replacement; that is, after drawing a chip, it was replaced and thoroughly mixed with the others before another was drawn. In the bowl there were 998 circular chips on each of which there was a number. Forty chips were marked 0, 40 were marked $-0{\cdot}1$, 40 were marked $+0{\cdot}1$, and so on, the classes differing by units of 0·1 and the frequencies in them being as nearly as possible normal. The range was from $-3{\cdot}0$ to $+3{\cdot}0$.

For our purpose the five samples of 25 were selected at random from the 4000 and the scale was then changed so that the scores could run from 0 to 100 instead.

Applying the theory to the expected values of the correlations gives us

$$g = 0{\cdot}20a + 0{\cdot}25b + 0{\cdot}31c + 0{\cdot}37d + 0{\cdot}60i. \qquad \text{......(ii)}$$

Applying the theory to the observed values gives

$$g' = 0{\cdot}09a + 0{\cdot}60b + 0{\cdot}28c + 0{\cdot}12d + 0{\cdot}40i. \qquad \text{......(iii)}$$

Using these values, we calculated

(α) $$g_1 = 0{\cdot}20a + 0{\cdot}25b + 0{\cdot}31c + 0{\cdot}37d,$$

i.e. the determinate part of g calculated from the expected values of the correlations (assuming i to have its average value zero).

(β) $$g_2 = 0{\cdot}09a + 0{\cdot}60b + 0{\cdot}28c + 0{\cdot}12d,$$

i.e. the determinate part of g calculated from the observed values of the correlations.

A sixth sample of values was then drawn at random and taken to represent i. After expressing the results in standard measure, these values were substituted in formulae (ii) and (iii) giving g_3 and g_4. The results are shown in Table II. We find

$$r_{Gg_1} = 0{\cdot}83, \qquad r_{Gg_2} = 0{\cdot}82, \qquad r_{Gg_3} = 0{\cdot}81, \qquad r_{Gg_4} = 0{\cdot}83$$[1].

The agreement must be regarded as satisfactory, but it must be noted that we may be badly out in individual cases, and the results give us an idea of the sort of accuracy with which we can estimate g from 25 subjects in practice. The agreement between G and g_1 or G and g_2 would not be improved materially by increasing the size of the sample; it would, however, be improved by increasing the number of tests. The agreement between g_1 and g_2, however, would improve with increasing size of sample. The same remarks apply to g_3 and g_4.

(3) The third difficulty which has also been brought to light by E. B. Wilson's work can be stated somewhat technically by saying that g is not necessarily invariant for linear transformations. To explain what we mean by this statement, let us suppose we have four tests a, b, c, d which satisfy the tetrad criterion. Now suppose we form four new tests artificially by taking weighted combinations of the scores in the original tests, *e.g.*

$$\left.\begin{aligned} a' &= W_1 a + W_2 b + W_3 c + W_4 d \\ b' &= W_5 a + W_6 b + W_7 c + W_8 d \\ c' &= W_9 a + W_{10} b + W_{11} c + W_{12} d \\ d' &= W_{13} a + W_{14} b + W_{15} c + W_{16} d \end{aligned}\right\}. \qquad \text{......(iv)}$$

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[1] The values of r_{Gg_3} and r_{Gg_4} are somewhat higher than might have been anticipated owing to the fact that our 25 values of G and i are positively correlated to the extent of $+0{\cdot}26$ owing to fluctuations of sampling.

We can suppose the weights chosen so that the standard deviations of the new scores are also unity. We may correlate a', b', c', d' and form new tetrad differences with these correlations. These new tetrad differences will not necessarily be zero, but it can be shown that they can be made zero by a suitable choice of weighting factors. Suppose they are made zero, then we can estimate a general factor g' from a', b', c', d'. It can be shown that this general factor g' will not necessarily be the same as g, though we can make it so by a suitable choice of combining weights. It may be shown in fact that if we write down the equations necessary to make the new tetrads vanish and to make the correlations of g' with a', b', c', d' have any assigned values we choose to give them, we shall have fewer equations than unknowns, so that many solutions would exist. That is to say, by linear transformation we can get a g' having any value we choose to give it. In particular, we might make all the correlations of g' with a', b', c', d' zero; in this case the new system would consist entirely of specific tests and they would have no general factor at all. In actual practice our freedom of choice would be somewhat less than this, because the equations to be solved are quadratic and may have no real solutions, so that a limitation of our choice would arise if we are to confine ourselves to real weighting factors. A further limitation would occur if we insisted on positive weights.

A numerical example may make the process clear. Suppose we have four tests a, b, c, d with correlations

$$r_{ab} = 0{\cdot}42,\quad r_{ac} = 0{\cdot}48,\quad r_{bc} = 0{\cdot}56,\quad r_{ad} = 0{\cdot}54,\quad r_{bd} = 0{\cdot}63,\quad r_{cd} = 0{\cdot}72.$$

These form a hierarchy with

$$r_{ag} = 0{\cdot}6,\quad r_{bg} = 0{\cdot}7,\quad r_{cg} = 0{\cdot}8,\quad r_{dg} = 0{\cdot}9.$$

Now take

$$\left.\begin{aligned} a' &= 0{\cdot}2420a + 0{\cdot}4839b + 0{\cdot}4839c \\ b' &= 0{\cdot}4885a + 0{\cdot}2443b + 0{\cdot}4885c \\ c' &= 0{\cdot}4921a + 0{\cdot}4921b + 0{\cdot}2460c \\ d' &= 0{\cdot}5141a + 0{\cdot}6214b + 0{\cdot}8383c - 1{\cdot}3043d \end{aligned}\right\} . \qquad \text{......(v)}$$

It may be shown that the standard deviations of a', b', c', d' are all unity and

$$\begin{aligned} r_{a'b'} &= 0{\cdot}9658, & r_{b'c'} &= 0{\cdot}9736, & r_{c'a'} &= 0{\cdot}9692, \\ r_{a'd'} &= 0{\cdot}5883, & r_{b'd'} &= 0{\cdot}5910, & r_{c'd'} &= 0{\cdot}5931. \end{aligned}$$

The tetrads will vanish and we find

$$r_{a'g'} = 0{\cdot}9805,\qquad r_{b'g'} = 0{\cdot}9850,\qquad r_{c'g'} = 0{\cdot}9884,\qquad r_{d'g'} = 0{\cdot}6000.$$

We can show that the g corresponding to a, b, c, d is:

$$g = 0{\cdot}1095a + 0{\cdot}1603b + 0{\cdot}2595c + 0{\cdot}5531d + 0{\cdot}3417i,$$

while g' corresponding to a', b', c', d' is given by

$$g' = 0{\cdot}4327a + 0{\cdot}4148b + 0{\cdot}3920c - 0{\cdot}0120d + 0{\cdot}0993i.$$

This shows very clearly that g and g' are quite different. The correlations between a', b', c' are considerably higher than would occur in practice. This is because in order to simplify the arithmetic to get a', b', c', weights were taken in the simple ratios

$$\begin{matrix} 1, & 2, & 2, \\ 2, & 1, & 2, \\ 2, & 2, & 1, \end{matrix}$$

and the weights in the expression for d' were then chosen so as to make $r_{d'g'} = 0{\cdot}6$. This example is purely illustrative of the principle that g is not necessarily invariant, so that the use of higher correlations than would occur in practice is immaterial.

To sum up the third difficulty we may say:

(i) Not all linear transformations of hierarchical systems will keep the tetrads zero.

(ii) Of those transformations which do keep the tetrads zero, not all will conserve g.

(iii) We can always find transformations which will conserve g.

Thus as E. B. Wilson has put it, "g is relative to the set-up"; whatever psychological importance may be attached to the existence of a general factor must be consistent with the fact that linear combinations of the scores of the tests chosen "may be meaningless." On the other hand, a particular psychological importance would appear to attach to those transformations which do conserve g.

Finally I should like to say this. The single-factor theory as I understand it is a statistical device for showing whether a set of scores have one common factor or not; but it can tell us nothing about the nature of the common factor if it exists. Such common factors might well exist in other than psychological fields—E. B. Wilson has, I believe, given an example in meteorology—and they may exist in psychological fields unconnected with intelligence, for instance, a common motor factor seems to have been found lately over a limited field of manual operations.

There is a certain analogy between the meaning of 'g' and the meaning of the correlation coefficient. A significant correlation coefficient tells us that association exists between two variables, but it tells us nothing about the reasons for the association. A significant '*g*' tells us that a series of tests has a common factor, but it does not tell us why. This has two consequences. First of all it should tend to make us careful of the inferences we draw from the fact of the existence of a common factor. On the other hand, the people who have found a '*g*' emerging from scores artificially constructed from combinations of dice scores have no more disproved the possible existence of 'general intelligence' or 'mental energy' than a man who, having observed a correlation between the number of hearts in his own hand and the number of spades in his partner's hand at bridge would have disproved that the correlation in stature between father and son was due to heredity.

I am indebted to my colleagues Mr J. Martin and Mr E. A. Cheesman for carrying out the computations connected with the sampling experiment.

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Table I. *Five random samples of 25 and the assumed test scores derived from them.*

										Standardized				
No.	*A*	*B*	*C*	*D*	*G*	*a**	*b**	*c**	*d**	*a*	*b*	*c*	*d*	*G*
1	78	55	57	58	32	63	46	47	46	1·0	−0·6	−0·4	−0·5	−0·9
2	53	55	57	38	62	56	58	59	49	0·4	0·4	1·1	−0·3	0·6
3	73	57	52	63	52	66	55	52	58	1·3	0·2	0·2	0·5	0·1
4	58	57	57	67	45	54	53	52	57	0·2	0·0	0·2	0·4	−0·3
5	33	50	47	85	57	41	53	51	73	−0·9	0·0	0·1	1·6	0·4
6	63	23	48	22	32	53	26	41	26	0·2	−2·3	−1·2	−2·0	−0·9
7	45	62	53	75	18	36	46	39	50	−1·3	−0·6	−1·4	−0·2	−1·6
8	53	72	58	92	27	44	55	45	63	−0·6	0·2	−0·7	0·9	−1·1
9	12	48	60	63	18	14	37	43	43	−3·2	−1·4	−0·9	−0·7	−1·6
10	53	50	20	15	73	60	59	42	41	0·8	0·5	−1·1	−0·9	1·2
11	60	65	57	42	48	56	59	53	45	0·4	0·5	0·4	−0·6	−0·1
12	63	52	42	65	48	58	51	44	57	0·6	−0·2	−0·8	0·4	−0·1
13	63	45	33	42	70	65	54	48	54	1·2	0·1	−0·3	0·1	1·0
14	43	63	38	58	98	61	76	63	76	0·9	2·0	1·7	1·9	2·4
15	63	67	47	62	63	63	66	54	62	1·0	1·1	0·5	0·8	0·7
16	42	55	58	70	47	44	52	53	60	−0·6	−0·1	0·4	0·6	−0·2
17	55	40	55	42	20	43	33	41	32	−0·7	−1·7	−1·2	−1·6	−1·5
18	43	83	43	27	75	54	80	56	48	0·2	2·3	0·8	−0·3	1·3
19	52	75	48	73	60	55	69	53	67	0·3	1·4	0·4	1·2	0·5
20	47	47	50	65	45	46	46	48	56	−0·5	−0·6	−0·3	0·3	−0·3
21	57	40	75	50	67	60	50	72	58	0·8	−0·3	2·8	0·5	0·9
22	35	38	40	70	55	42	44	46	63	−0·8	−0·8	−0·5	0·9	0·3
23	60	67	55	33	18	46	49	40	26	−0·5	−0·3	−1·3	−2·0	−1·6
24	58	45	45	53	63	60	52	52	57	0·8	−0·1	0·2	0·4	0·7
25	32	58	63	23	55	40	57	60	37	−1·0	0·3	1·3	−1·2	0·3

* $a=(A+0{\cdot}5G)/1{\cdot}5$; $b=(B+0{\cdot}6G)/1{\cdot}6$; $c=(C+0{\cdot}7G)/1{\cdot}7$; $d=(D+0{\cdot}8G)/1{\cdot}8$.

Table II. *Comparison between assumed observed values of g and values obtained from the theory (scores standardized).*

			Theoretical values			
No.	i	G (observed)	g_1	g_2	g_3	g_4
1	1·2	−0·9	−0·3	−0·4	0·5	0·0
2	0·4	0·6	0·5	0·6	0·7	0·7
3	1·7	0·1	0·6	0·3	1·6	1·0
4	−1·3	−0·3	0·3	0·1	−0·6	−0·4
5	0·7	0·4	0·5	0·2	0·9	0·4
6	−0·4	−0·9	−1·7	−1·9	−1·9	−2·1
7	−1·4	−1·6	−0·9	−0·9	−1·8	−1·5
8	−2·0	−1·1	0·0	0·0	−1·2	−0·8
9	−0·4	−1·6	−1·6	−1·5	−1·8	−1·6
10	1·0	1·2	−0·4	0·0	0·3	0·4
11	−0·1	−0·1	0·1	0·4	0·1	0·3
12	−0·8	−0·1	0·0	−0·2	−0·5	−0·5
13	−0·4	1·0	0·2	0·1	0·0	−0·1
14	1·2	2·4	1·9	1·9	2·6	2·4
15	−0·5	0·7	0·9	1·0	0·6	0·8
16	0·2	−0·2	0·2	0·1	0·3	0·1
17	1·0	−1·5	−1·5	−1·6	−0·9	−1·2
18	0·4	1·3	0·8	1·6	1·0	1·7
19	0·4	0·5	1·0	1·1	1·2	1·2
20	0·1	−0·3	−0·2	−0·4	−0·2	−0·4
21	−2·4	0·9	1·1	0·8	−0·3	−0·2
22	−0·2	0·3	−0·2	−0·6	−0·3	−0·6
23	0·1	−1·6	−1·4	−0·9	−1·3	−0·8
24	1·0	0·7	0·4	0·1	1·0	0·5
25	0·5	0·3	−0·2	0·3	0·2	0·5

APPENDIX TO SECTION (3).

A. *The transformation represented by equations* (iv).

In general, if we have k tests we shall have k^2 unknown coefficients to determine. The condition that the new standard deviations should be equal to unity, provides k equations, while the new tetrad relations provide $\frac{k(k-3)}{2}$ further independent equations. This may be seen by noting that the tetrad relations are equivalent to the $\frac{k(k-1)}{2}$ equations of the type $r_{a'b'} = r_{a'g'} r_{b'g'}$; the elimination of the k quantities $r_{a'g'}, r_{b'g'}, \ldots$ leaves us with $\frac{k(k-3)}{2}$ independent equations. Thus if we assign any values to $a', b', c', \ldots$, the equations can be satisfied with

$$k^2 - \frac{k(k-3)}{2} - k = \frac{k^2+k}{2}$$

degrees of freedom. If $k = 4$ this amounts to 10 degrees of freedom. g' can thus be made to have any value we please by an appropriate choice of coefficients, but for given values of g and g' some of the coefficients may be negative or imaginary.

B. *The coefficients in equations* (v).

The coefficients in equations (v) were obtained as follows: It was decided to take

$$\left.\begin{aligned} a' &= \frac{1}{\sqrt{17 \cdot 08}}(a + 2b + 2c) \\ b' &= \frac{1}{\sqrt{16 \cdot 76}}(2a + b + 2c) \\ c' &= \frac{1}{\sqrt{16 \cdot 52}}(2a + 2b + c) \end{aligned}\right\}. \qquad \ldots\ldots\text{(i)}$$

Thus the coefficients are in simple ratios, and the denominators of the expressions on the right-hand side of (i) are chosen to make the standard deviations of a', b', c' unity.

$r_{a'b'}$, $r_{b'c'}$, $r_{c'a'}$ were at once determinable, and from these $r_{a'g'}$, $r_{b'g'}$, $r_{c'g'}$ were obtained by means of relations of the type

$$r_{a'g'} = \sqrt{\frac{r_{a'b'} \cdot r_{a'c'}}{r_{b'c'}}}.$$

The values $r_{a'g'} = 0 \cdot 9805$, $r_{b'g'} = 0 \cdot 9850$, $r_{c'g'} = 0 \cdot 9884$ were found. It was decided to take $r_{d'g'} = 0 \cdot 6$. Now if we write

$$\left.\begin{aligned} a' &= w_1 a + w_2 b + w_3 c \\ b' &= w_4 a + w_5 b + w_6 c \\ c' &= w_7 a + w_8 b + w_9 c \\ d' &= w_{10} a + w_{11} b + w_{12} c + w_{13} d \end{aligned}\right\}, \qquad \ldots\ldots\text{(ii)}$$

where $w_1, \ldots, w_9$ have the numerical values given in (i), the problem is to determine w_{10}, w_{11}, w_{12}, w_{13} so as to satisfy the new tetrad relations. Now we know

$$\begin{aligned} r_{a'd'} &= r_{a'g'} r_{d'g'} = 0 \cdot 5883, \\ r_{b'd'} &= r_{b'g'} r_{d'g} = 0 \cdot 5910, \\ r_{c'd'} &= r_{c'g'} r_{d'g'} = 0 \cdot 5931, \end{aligned}$$

and equations (ii) give us

$$\left.\begin{aligned} A_1 w_{10} + B_1 w_{11} + C_1 w_{12} + D_1 w_{13} &= r_{a'd'} = 0 \cdot 5883 \\ A_2 w_{10} + B_2 w_{11} + C_2 w_{12} + D_2 w_{13} &= r_{b'd'} = 0 \cdot 5910 \\ A_3 w_{10} + B_3 w_{11} + C_3 w_{12} + D_3 w_{13} &= r_{c'd'} = 0 \cdot 5931 \end{aligned}\right\}, \qquad \ldots\ldots\text{(iii)}$$

where

$$\left.\begin{aligned} A_1 &= w_1 + w_2 r_{ab} + w_3 r_{ac} \\ B_1 &= w_1 r_{ab} + w_2 + w_3 r_{bc} \\ C_1 &= w_1 r_{ac} + w_2 r_{bc} + w_3 \\ D_1 &= w_1 r_{ad} + w_2 r_{bd} + w_3 r_{cd} \end{aligned}\right\}, \qquad \text{......(iv)}$$

A_2, B_2, C_2, D_2 are obtained by substituting w_4, w_5, w_6 respectively for w_1, w_2, w_3 in equations (iv) and A_3, B_3, C_3, D_3 by a similar substitution of w_7, w_8, w_9.

Now let

$$\left.\begin{aligned} a_1 &= \sqrt{17{\cdot}08}A_1, & a_2 &= \sqrt{16{\cdot}76}A_2, & a_3 &= \sqrt{16{\cdot}52}A_3 \\ b_1 &= \sqrt{17{\cdot}08}B_1, & b_2 &= \sqrt{16{\cdot}76}B_2, & b_3 &= \sqrt{16{\cdot}52}B_3 \\ c_1 &= \sqrt{17{\cdot}08}C_1, & c_2 &= \sqrt{16{\cdot}76}C_2, & c_3 &= \sqrt{16{\cdot}52}C_3 \end{aligned}\right\}. \qquad \text{...(v)}$$

Then

$$\left.\begin{aligned} a_1 &= 1 + 2r_{ab} + 2r_{ac} \\ b_1 &= r_{ab} + 2 + 2r_{bc} \\ c_1 &= r_{ac} + 2r_{bc} + 2 \\ d_1 &= r_{ad} + 2r_{bd} + 2r_{cd} \end{aligned}\right\}. \qquad \text{......(vi)}$$

a_2, b_2, c_2, d_2 are obtained by substituting the coefficients 2, 1, 2 respectively for 1, 2, 2 in the equations (vi), while a_3, b_3, c_3, d_3 are obtained by substituting the coefficients 2, 2, 1.

With the values of r_{ab}, r_{bc}, r_{ca} given in the text, we find

$$\begin{aligned} a_1 &= 2{\cdot}80, & b_1 &= 3{\cdot}54, & c_1 &= 3{\cdot}60, & d_1 &= 3{\cdot}24, \\ a_2 &= 3{\cdot}38, & b_2 &= 2{\cdot}96, & c_2 &= 3{\cdot}52, & d_2 &= 3{\cdot}15, \\ a_3 &= 3{\cdot}32, & b_3 &= 3{\cdot}40, & c_3 &= 3{\cdot}08, & d_3 &= 3{\cdot}06. \end{aligned}$$

We thus reach the equations:

$$\begin{aligned} 2{\cdot}80w_{10} + 3{\cdot}54w_{11} + 3{\cdot}60w_{12} + 3{\cdot}24w_{13} &= 2{\cdot}4314, \\ 3{\cdot}38w_{10} + 2{\cdot}96w_{11} + 3{\cdot}52w_{12} + 3{\cdot}15w_{13} &= 2{\cdot}4194, \\ 3{\cdot}32w_{10} + 3{\cdot}40w_{11} + 3{\cdot}08w_{12} + 3{\cdot}06w_{13} &= 2{\cdot}4105, \end{aligned}$$

or

$$\left.\begin{aligned} w_{10} &= 0{\cdot}2582 - 0{\cdot}1962w_{13} \\ w_{11} &= 0{\cdot}2468 - 0{\cdot}2872w_{13} \\ w_{12} &= 0{\cdot}2318 - 0{\cdot}4650w_{13} \end{aligned}\right\} \qquad \text{......(vii)}$$

Finally, the fact that the standard deviation of d' must be unity gives rise to the equation

$$\begin{aligned} w_{10}^2 + w_{11}^2 + w_{12}^2 + w_{13}^2 + 2w_{10}w_{11}r_{ab} + 2w_{10}w_{12}r_{bc} + 2w_{10}w_{13}r_{ad} \\ + 2w_{11}w_{12}r_{bc} + 2w_{11}w_{13}r_{bd} + 2w_{12}w_{13}r_{cd} = 1. \qquad \text{......(viii)} \end{aligned}$$

Substituting the values (vii) in (viii) gives us a quadratic in w_{13} in which the coefficient of w_{13} can be shown to vanish.

We find $$w_{13} = \pm 1\cdot3043, \qquad \text{......(ix)}$$

leading to two solutions:

$$\left.\begin{array}{l} w_{10} = +0\cdot5141 \\ w_{11} = +0\cdot6214 \\ w_{12} = +0\cdot8383 \\ w_{13} = -1\cdot3043 \end{array}\right\} \text{......(x)} \quad \text{and} \quad \left.\begin{array}{l} w_{10} = +0\cdot0023 \\ w_{11} = -0\cdot1278 \\ w_{12} = -0\cdot3746 \\ w_{13} = +1\cdot3043 \end{array}\right\}. \qquad \text{......(x } bis\text{)}$$

Using equations (i) of the text (x) gives us

$$g' = 0\cdot4327a + 0\cdot4148b + 0\cdot3920c - 0\cdot0120d + 0\cdot0993i',$$

while (x *bis*) gives us

$$g' = 0\cdot4280a + 0\cdot4079b + 0\cdot3808c + 0\cdot0120d + 0\cdot0993i''.$$

Both of these are essentially different from g, for

$$g = 0\cdot1095a + 0\cdot1603b + 0\cdot2595c + 0\cdot5531d + 0\cdot3417i.$$

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THE EFFECT OF DELAYED REWARD ON THE MAZE PERFORMANCE OF CHICKENS.

BY A. P. CHATTOCK AND G. C. GRINDLEY.

I. *Introduction* (pp. 382–383).
II. *Experimental procedure and treatment of results* (pp. 383–384).
III. *Results* (pp. 384–387).
IV. *Further experiments* (p. 387).
V. *Summary* (p. 387).
References (*p.* 388).

I. INTRODUCTION.

THE way in which trial and error learning, or the performance of a habit which has been acquired by trial and error learning, is influenced by a period of delay between the performance of a correct response and the moment when the animal obtains the reward is a problem of some interest in connection with the theory of learning. From theories such as those underlying Thorndike's "Law of Effect (3)," it might be expected that the effect of the reward in 'stamping in' the correct response would become less as the period of delay between the response and the reward is made longer.

There is not very much experimental evidence on this question, and different workers (all working with rats, but employing different experimental conditions) have been led to opposite conclusions. Watson (6), using a problem box and a delay of 30 sec., Simmons (2), using a maze and a delay of about 2 hours, and Warden and Haas (5), using a maze and delays of 1 and 5 min., all found that the delay had no appreciable effect on the rate of learning. On the other hand, Hamilton (1), in a carefully controlled experiment, found that a delay of 1 min. produced a marked decrease (about 50 per cent.) in the rate at which a maze was learnt. Hamilton also found, using the "Columbia Obstruction Method" (Warden (4)), that the hunger drive, as measured by the frequency with which a rat would cross an electric grid to obtain food, was markedly decreased by a delay as short as 15 sec. before feeding. It is difficult to see the reason for these differences in the results of different workers, and further experimental work on this problem seems to be needed.

The present paper describes experiments on the effect of periods of delay of 30, 60 and 150 sec. on the maze performance of chickens. The experiments dealt with the influence of delayed feeding on the performance of chicks which had reached the horizontal portion of the learning curve, rather than with the effect of delayed reward on the initial learning.

II. EXPERIMENTAL PROCEDURE AND TREATMENT OF RESULTS.

The apparatus used was very simple, but it will be referred to as a 'maze,' since the general principle of the experiment was similar to that of a maze experiment. Fig. 1 is a plan of the apparatus, the run *ABCD* being about 30 in. square, and being surrounded by walls about 1 ft. high. The 'release-box,' *RB*, and the 'food-box,' *FB*, had sliding doors, D_1 and D_2, which could be operated by the experimenter.

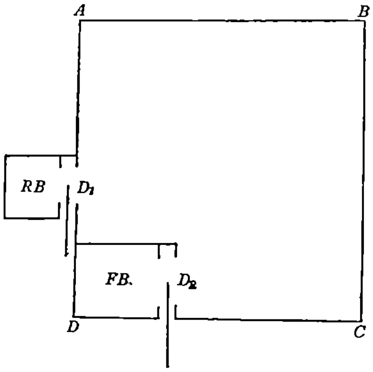

Fig. 1.

Each chick ran the maze, from *RB* to *FB*, once on each day, in the morning before it had been fed. At each trial a chick was given a score equal to $\log_{10} \frac{100}{t}$, where t was the time in seconds from the moment when the door of the release-box was opened to the moment when the chick entered the food-box. When the chick entered the food-box, the door D_2 was closed; and, either immediately or after any desired period of delay, a reward of four grains of boiled rice was presented to the chick on a small tray which slid under one of the walls of the food-box.

The chicks were taught to run the maze for an immediate reward until the average learning curve became approximately horizontal. This usually occurred after about 14 days. They were then divided into two groups, A and B, of as nearly as possible equal average scores. Group A continued to run the maze for a further number of days (usually six) with an immediate reward; while group B had a period of delay (*e.g.* 1 min.) in the food-box before the reward was given. The treatment of the two groups was then reversed, group B running the maze for a further 6 days with an immediate reward, while group A had a delay of 1 min. The treatment of the two groups was reversed in this way every 6 days for some weeks, usually until the birds became too large to pass easily through the doors.

It is clear that if the delay has a depressing effect on maze performance, the average score of group A (which will be called S_A) should rise relatively to that of group B (S_B), during the first 6-day period, and that S_B should rise relatively to S_A during the second 6-day period, and so on. (Actually, the change in the relative score of the two groups would be expected to be in one direction during the last 5 days of each of the 6-day periods *and* on the first day of the next 6-day period, and then in the opposite direction until the first day of the following 6-day period, since the changes in the reward conditions cannot affect the chicks' scores on the actual days on which the changes are made.) A graph showing the value of S_A–S_B should therefore rise from the second to the seventh day after the beginning of the differential treatment of the two groups, and a graph showing the value of S_B–S_A should rise for the following 6 days, and so on. The slope of the first curve should be less steep than that of the subsequent curves, since in the first curve the reward conditions of only one of the groups have been changed, while in the subsequent curves the changed conditions of reward for both groups should contribute to the upward slope. The average of all these curves, except the first, was plotted in each experiment.

It will be seen that this method of treatment of the results eliminates, as far as possible, the 'chance' variations in the scores of individual chicks, and that it also eliminates errors due to influences (such as changes in weather conditions) which acted on all the chicks.

III. RESULTS.

Six experiments were made in the way which has been described, and the data for these are summarised in Table I, while the average curves (which should all slope upwards if the delay depresses the maze performance) for each experiment are shown in Fig. 2. It will be seen that all the curves have an upward tendency; and hence that the results indicate that periods of delay of 30, 60 and 150 sec. all depress the maze performance of chickens under these conditions.

Table I.

(1) Exp.	(2) Number of birds per group	(3) Age at end (days)	(4) Delay period (sec.)	(5) Days between successive reversals	(6) Number of reversals	(7) Average score for each group	(8) Effect of delay
1	7 and 6	54	30	6	6	1·28 and 1·18	0·14
2	6 „ 6	61	30	6	4	1·30 „ 1·22	0·07
3	6 „ 6	47	60	6	1	1·14 „ 1·03	0·14
4	6 „ 8	37	60	7	2	1·06 „ 1·04	0·12
5	9 „ 9	50	60	8	2	1·13 „ 1·35	0·24
6	6 „ 6	61	150	6	6	1·28 „ 1·05	0·13

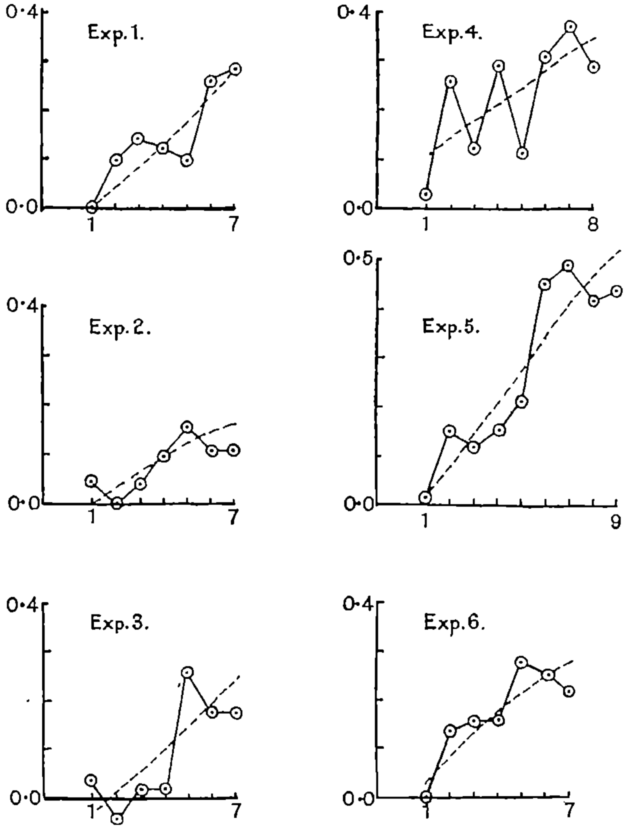

Fig. 2. Abscissae—Days after reversal. Ordinates—Difference in score.

But the fluctuations in the curves are so great as to make impossible accurate measurement of the effect of delay. A rough estimate may, however, be made by drawing smooth curves (the dotted curves in Fig. 2) through the points, and measuring the rise in these. One-half of this quantity represents roughly the average depressing effect of the delay when the delay has acted for the number of days between reversals (shown in column (6) of Table I), since the curves represent the difference in score between two groups, the performance of one of which is being depressed by the delay, while the performance of the other is being improved by a return to conditions of immediate reward.

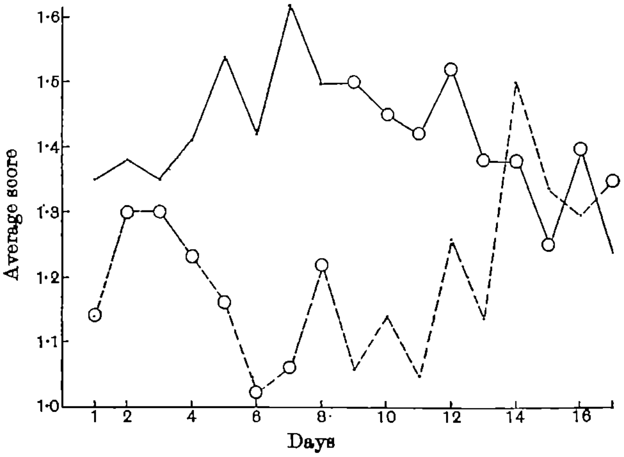

Fig. 3. Continuous line—Group A. Broken line—Group B. Circles indicate delay before feeding.

The figures in column (7) give the average scores of all chicks during each experiment, and the figures in column (8) show the effect of delay, as deduced from the curves in Fig. 2. It can be estimated, from the data of Exps. 1 and 2, that a delay of 30 sec., acting for 6 days, produces a depression of about 0·1 in the average score. That is to say, on the average, a delay of 30 sec., acting for 6 days, increases the time taken by the chicks to run the maze from about 6 sec. to about 7·5 sec. The other experiments suggest that longer periods of delay have a greater effect than this, but the data are not sufficient to show how the effect varies with the length of the delay period.

The behaviour of the chicks during the experiments did not show any unusual features. There were wide differences in performance between individual chicks, and even the average curves for groups of six or eight birds were far from regular. Fig. 3, which is typical, shows a portion of the performance curves of the two groups of birds used in Exp. 5. The circles indicate the days on which each group had a period of delay (60 sec.) before feeding.

IV. FURTHER EXPERIMENTS.

A few more experiments, which need not be described in detail, were made to test whether the chicks' performance in running the maze was influenced by the place where the chicks spent the delay period. It was thought possible that chicks which spent this period in the food-box (which would, presumably, be strongly associated with food in the chicks' minds) might show a better performance than chicks which spent the period in another box and were only allowed to enter the food-box at the end of the delay. The results suggested that this effect actually occurred, but that the maze performance was also influenced by several other factors, such as the illumination of the place where the delay was spent, and the possibility of seeing into the food-box from the other box. The chicks appeared to show a better maze performance if the delay was spent in a well-illuminated box than if it was spent in a poorly illuminated box, and to show a better performance if they could see into the food-box than if they could not do so. But these factors were not adequately controlled, and the results of the experiments were therefore inconclusive.

The results do, however, suggest that maze performance with a delayed reward is affected by other influences besides the amount of the reward and the length of the delay period, and it seems to be very probable that some of the apparent contradictions between the results of different experiments on delayed reward (cf. Section I) may be due to factors of this kind.

V. SUMMARY.

1. Evidence is given that the performance of chicks in a very simple maze is depressed if, after the maze has been run, there is a period of delay of 30, 60 or 150 sec. before the food reward is given. Under the conditions of these experiments, a delay of 30 sec. in six successive trials increases the average time taken to run the maze by about 25 per cent.

2. Further experiments suggest that maze performance is influenced, not only by the length of the delay period, but also by other influences acting on the animal during the delay period.

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A REPETITION OF RUBIN'S FIGURE-GROUND EXPERIMENT.

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The following experiment was carried out in order to retest the effect on recognition of figure dominance reported by Rubin[1]. The materials used were similar to his, and, in some cases, identical. A few representative cards are illustrated in Fig. 1. These figures can be seen in either of two ways: (1) as white figures on a black ground, or (2) as black silhouettes against a white ground or as 'holes' in the black figure.

The method of procedure was as follows: a series of twenty-four cards was prepared; twelve of these were exposed in a tachistoscope (time of exposure 0·23 sec.) at the first sitting. The subject was instructed to see the first six cards as white figures, the second six as 'holes.' The order of presentation for each set of six was altered for each subject. After an interval of fifteen minutes, three of the cards which had been shown as white figures and three which had been shown as 'holes' were mixed with six new cards, and all twelve exposed in the tachistoscope; the subject was instructed to see these as white figures and to say which he recognized. This series was immediately followed by another containing the remaining six cards from the first series and six new ones, which the subject was instructed to see as 'holes.' Recognition was again required. Twenty subjects were tested in this manner.

Rubin's results were, briefly, that when the instructions were to see a card as a white figure both at the first exposure and in the second series, recognition was more likely than when the card was re-exposed as a 'hole.' Conversely, cards exposed at the first sitting as 'holes' were more frequently recognized when re-exposed in the same manner.

In attempting to verify this conclusion, our first problem was to produce the required types of perception. No difficulty was encountered with the perception of white figures; but many subjects found it difficult to see the cards as 'holes.' Two subjects who followed instructions very easily established a particular attitude toward the cards to be seen as

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[1] Rubin, E. (1925). *Visuell wahrgenommene Figuren.* Copenhagen: Gyldendal.

'holes.' They thought of them 'as some object,' and then looked at the cards from that point of view. One of the subjects saw them as 'stencils,' the other as the 'mouth of a cave, with the sea beyond.' These ideas we borrowed and suggested to aid subjects who found 'hole' perception difficult—in some cases the idea of 'cave-mouth,' in others that of 'stencil' gave most help. In a number of instances we introduced a trial card before the set of cards to be seen as 'holes.' Even then the success

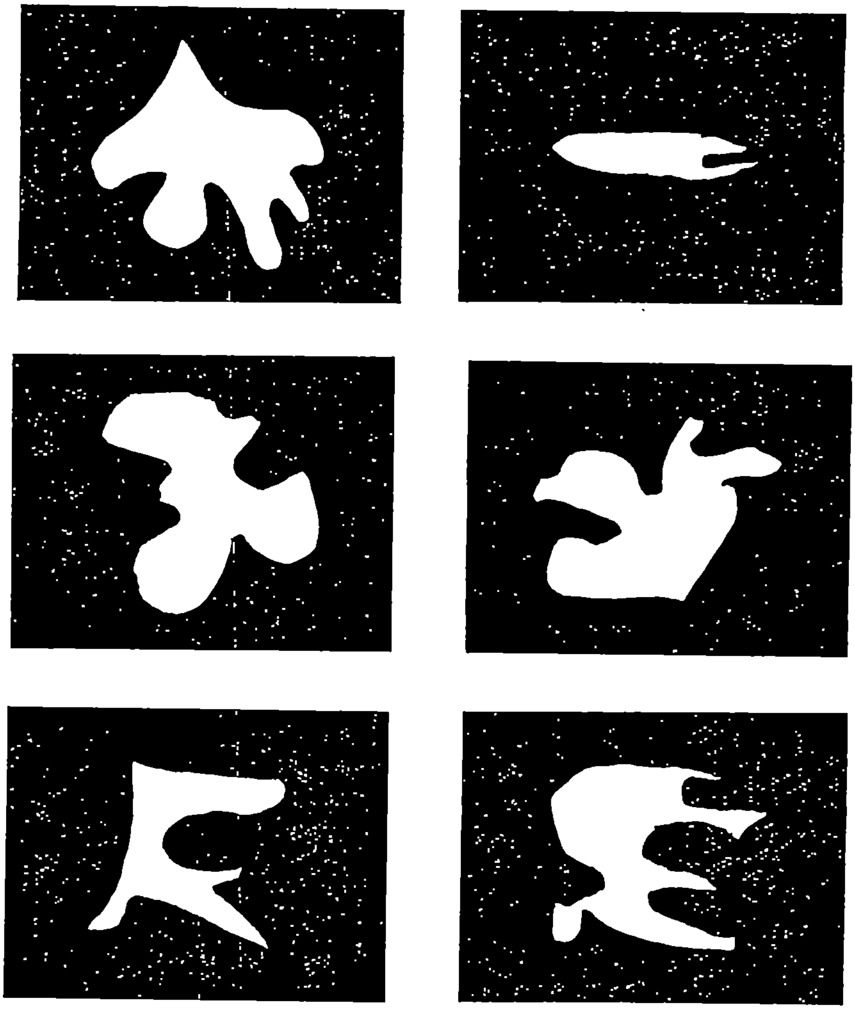

Fig. 1.

attained in seeing the white as a background to a hole in the black was far from complete. The difficulty with the 'hole' perception will be discussed below. For purposes of calculation the manner in which the card actually *was* seen has been used. Ambiguous cases in which the card was seen first as a white figure and then as a 'hole,' or *vice versa*, have been omitted[1].

Table I gives the number of cards which should have been, and the number which actually were, seen in the different ways.

Table I.

1st series	Instructions to see white figure	120	White seen	156
	Instructions to see hole	120	Hole seen	72
			Ambiguities	12
		240		240
2nd series	Instructions to see white figure	120	White seen	143
	Instructions to see hole	120	Hole seen	90
			Ambiguities	7
		240		240

The second or recognition series, then, will include in our calculations, 221 cases of cards also exposed in the first series, instead of 240 (12 cards, 20 subjects). Twelve omissions are due to ambiguities in the first presentation, seven in the second. The total of new cards introduced into the second series was 233 (seven cards omitted because of their use as trial cards).

New cards	Instructions to see white figure	117	White seen	135
	Instructions to see hole	116	Hole seen	89
			Seen both ways	9
		233		233

A summary of the results showing the relation of recognition to the manner in which the cards were seen, taking no account at present of the new cards in the second series, is given in Table II.

Table II.

	When the card was seen in:			
	The same manner in both series		A different manner in the 2nd series	
	I As white	II As hole	III As white in 1st, hole in 2nd	IV As hole in 1st, white in 2nd
Recognition occurred in	50	10	14	12
No recognition occurred in	23	11	26	14
Doubt occurred in	23	11	15	12
Total	96	32	55	38 = 221

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[1] See Appendix.

Considering columns I and III on the one hand, and II and IV on the other, we appear to be dealing with different situations. The drop in recognition in III as compared with I seems to indicate an effect on recognition of the difference in the way the card was seen in the two series. This result is not paralleled in column IV as compared with column II. This is a second suggestion—corroborating the initial difficulty in 'hole' perception—that seeing the white as a figure is a mental process in a different category from that of seeing it as a 'hole,' that the two are not on a comparable footing. Testing for a significant difference in power of recognition as between those cards seen in like and unlike manner on the second exposure, if we add columns I and II together and III and IV, we find $\chi^2 = 9{\cdot}37$ with a probability of one in a hundred—bordering on a significant difference. If the significance is calculated for columns I and III alone, $\chi^2 = 14{\cdot}03$ and the probability is 0·0009—an extremely small and significant value. (A direct comparison of the ability to recognize the cards when seen as white figures and as 'holes,' made by comparing the figures in columns I and II, at least points toward the same difference of status between the two types of perception. Testing for a significant difference between the columns, we get $\chi^2 = 4{\cdot}15$ and the probability 14 in a 100 that this difference should occur by chance. This result, though, may be of little value since we always presented the white figure cards first at the first sitting—a point in method which we regret since it is recognized that the first things seen are most easily remembered.)

Before accepting, for the white figures, a significant effect on recognition of the manner in which they are seen on the second occasion (that is, whether they are seen in the same or a different way), it is necessary to consider the number of false recognitions among the new cards or the number of recognitions we should expect by chance. Of the total of 233 new cards, 47 were erroneously recognized: 28 of the 135 seen as white figures, 17 of the 89 seen as 'holes,' 2 of the 9 seen in both ways. Comparing the percentages falsely recognized with the percentages of those correctly recognized, we find recognition of the white figures and 'holes' as shown in Table III.

Table III.

White figure:	Correct	52·0 %	Hole:	Correct[1]	25·4 %
	False	20·7 %		False	19·1 %
	Difference	31·3 %			6·3 %

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[1] "Hole: correct" indicates number of correct recognitions when the card was seen as a white figure in the first and as a 'hole' in the second series.

Subtracting the percentage of false recognitions from that of true recognitions we have a difference of 31·3 to 6·3 per cent., still a reasonably large difference.

To discriminate more precisely the causes of this difference of recognition several problems must be investigated. It is not enough to say that there was a change from a white figure to a white 'hole' in one case and not in another. In the first place, the mere alteration did not have the same effect when the sense of change was reversed. Both the initial difficulty of seeing the white as a 'hole,' and the difference in ability to recognize figure and 'hole,' suggest that a more careful analysis of the effect of instructions is needed. It may be that it was the strain of seeing the white as a 'hole' which was the dominant factor in making difficult the continuity between a past and present presentation necessary for recognition. This strain could be active whether or not the effort to see a 'hole' was successful. In the first series there were 37 cases where a white figure was seen when the instructions were to see a 'hole,' and one where the reverse order occurred. Of the 37, one produced ambiguous results in the second series and five were correlated with a similar inability to follow instructions to see a 'hole' in the second series. Considering the remaining 31 we find the distribution in Table IV.

Table IV.

	As white in 2nd series	As hole in 2nd series
Recognized	8	6
Not recognized	8	6
Doubtful	3	—

The figures are small, but it is interesting to compare the ratio 8 : 8 : 3 where the card has been seen as a white figure in both series with the ratio 32 : 13 : 10 for the similar situation differing only in the fact that in this case instructions were followed in the first as well as in the second series. The five cases of overlap in which instructions to see a 'hole' were not followed in either series produced two cases of recognition and three doubtful answers. Of the other 30 cases where instructions were not followed in the second series, two were preceded in the first series by perceptions in which it was impossible to specify whether or not a white figure or 'hole' had been seen. The remaining 28 cases were distributed as in Table V.

Figures are again small, and the indication contained in the first column must depend on how we interpret doubtful answers. The difference, however, between the recognized cards and the sum of the doubtful and unrecognized ones is proportionately less than in the cases where instructions were followed. It would seem then, that the effort to see a 'hole' even when not successful—so that there was no difference of perception in the two series—was a factor upsetting recognition. If this is true, the lesser efficiency in recognizing a 'hole' when it was seen before as a 'hole' becomes more comprehensible. For we are much more familiar with the general setting, white figure on black ground, than with a black expanse with a 'hole' in it; or again with a ground surrounding a figure, than with a figure surrounding a ground. So we may assume that the 'holes' probably introduced an initial strain. But if we adopt this explanation as one of importance in accounting for the difference between the recognition of white figures and of 'holes,' for the results when instructions are not followed, and for the tendency for—but by no means uniform phenomenon of—non-recognition of cards seen as white figures in the first but as 'holes' in the second series, we are faced with two problems.

Table V.

	As white in 2nd series		As hole in 2nd series	
	White in 1st	Hole in 1st	White in 1st	Hole in 1st
Recognized	8	2	0	0
Unrecognized	0	7	2	1
Doubtful	7	0	0	1

The first is: How does the upsetting strain work? Is there any tendency for something which is 'easy' or conversely for something 'difficult' to be recognized spontaneously or without justification? And secondly, the general question: Is the 'strain' another factor affecting recognition besides a difference in figure dominance? Are they independent or two aspects of the same thing? What evidence have we for the influence of figure dominance apart from strain?

With regard to the first question, the table of false recognitions of new cards given in Table VI can be considered from two view points.

Table VI.

	According to instructions		Against instructions		Ambiguous	
	Total	Falsely recognized	Total	Falsely recognized	Falsely recognized	Total all cases
Seen as white figures	112	24	23	4	—	135
Seen as holes	85	16	4	1	—	89
Seen ambiguously	—	—	—	—	2	9
	197	40	27	5	2	233

Taking the white figures as *prima facie* 'easy' and the 'holes' as 'difficult,' we can compare the ratios 28/135 and 17/89 or 0·207 and 0·191. The difference is negligible. This we should expect partly from the deviations from the instructions given, in both the white figure and 'hole' series, as seen in the table above; and partly from a consideration of individual differences. For instance, Subject 16 remarked that "all the white figures look familiar" and in fact she 'recognized' all but one of those she saw as white figures (and that one should have been recognized). Here the 'ease' of seeing the figure seems to have produced a general attitude of familiarity, whereas the 'strain' of seeing the 'hole' tended to further discrimination. On the other hand, Subject 18 commented that all the 'holes' looked familiar, and of the four she saw as 'holes' in the second series (both new and old) she recognized all. There may, then, as a result of individual differences, be tendencies to recognize spontaneously a white figure or a 'hole' but there is no general bias evident.

The difficulty in recognizing 'holes,' however, was imputed above, tentatively, to the strain of instructions acting against immediate prepossession in perception. A consideration of Table VI as instructions were or were not followed might give an indication from another angle of the action of strain on recognition. If we compare the ratio of falsely recognized cards to the total when instructions were followed, 40/197, with the ratio of those when instructions were not followed, 5/27, or 0·203 with 0·185, we find again an insignificant difference. The direction of the insignificant difference is, however, the same in this as in the former case, namely toward a lesser recognition of cases that are presumed to involve more strain. But from these data we cannot conclude that either instructions to see a 'hole,' or failure to follow instructions, are operative in causing or preventing false recognition. (There is, of course, a further possibility; there may be strain present, but not evident, in those cases where instructions were followed independently of whether a white figure or a 'hole' was perceived. In this particular experiment we have no way of determining how far this was true.) On the basis of the data we have, however, we should hazard a guess that a conflict in perception, probably either on the first or later occasion of presentation, decreases the efficiency of recognition, not directly by producing *ipso facto* a judgment (or feeling, etc.) of unfamiliarity, but by disturbing the subjective condition which assimilates the second to the first perception. And this conflict may have overt expression—as when instructions were not followed—or be presumptive—as when the majority of subjects reported difficulty, especially in the first series, in seeing a 'hole.'

With the material we used in this experiment, it is impossible to give a definite answer to the second question (is strain another factor, separable from change in figure dominance, hampering recognition?), since all subjects experienced some strain in seeing the 'hole.' The factors of strain and figure dominance, therefore, could not be accurately separated. A comparison of results (discussed above, and reproduced in the Table VII) for the whole group, and the similar results for the group when those subjects who had the *greatest* difficulty in following instructions are eliminated, is interesting but not conclusive.

If we consider that a subject had difficulty when he failed to follow instructions six times altogether, either in the first or second series, six subjects (1, 2, 4, 10, 12, 20) will fall into this (admittedly arbitrary) category.

Table VII.

	Whole group		Partial group	
	χ^2	P	χ^2	P
1. Significance of difference in recognition between cards seen in same or different way	9·37	0·01	5·05	0·08
2. Significance of difference in recognition between cards seen in same or different way for those seen as white in first series	14·03	0·0009	8·63	0·0145
3. Significance of type of figure seen—whether white or 'hole'	4·85	0·14	6·90	0·03
4. Percentage difference between correct and false recognitions of cards seen as white in first series when seen in second series in:	Same way 31·3 %	Different way 6·3 %	Same way 36·6 %	Different way 10·5 %

It will be seen that for those subjects who had least difficulty in following instructions, the importance of a change in the manner of perception was rather less and the importance of the type of figure seen was rather more than for the whole group. In other words, seeing a 'hole,' which by all indications is a strain, is of greater significance when the subject is most capable of forcing himself to follow instructions; and the condition of a 'change in the manner seen' is of lesser importance when the subject has greater facility in seeing things as required. The contradiction of 'forcing oneself' and 'facility' may invalidate such an interpretation, but we are inclined to think it is only apparent. The subject who can follow instructions is likely to have greater facility and because of that facility will be more capable of forcing and controlling himself. One would expect these two aspects of 'obedience' to be brought out according as we consider cases where difficulty is at a maximum ('hole' in both series compared with white in both) or cases where it is not so great ('hole' in only one series, especially the second). The data, as we have said before, are too restricted to warrant an hypothesis; but it seemed worth while to mention that when the most marked cases of strain are eliminated, the significance of χ^2, particularly with regard to the difference between cards seen as white in both series and as white in first and 'hole' in the second, was very considerably reduced.

Some remarks on individual cases may be interesting, though not entirely relevant to the problem of the influence of figure-hole perception on recognition. Table VIII gives the number of correct and false recognitions for each subject.

Table VIII.

Subject	Correct recognition	False recognition	Subject	Correct recognition	False recognition
1	1	1	11	3	4
2	4	2	12	1	3
3	4	0	13	6	2
4	4	0	14	3	4
5	3	0	15	6	2
6	5	0	16	7	7
7	5	2	17	8	3
8	3	1	18	3	5
9	4	2	19	6	4
10	6	3	20	4	2
			Totals	86	47

No close correlation between true and false recognition appears. It should be mentioned that the compilation of the cases of false recognition had to be selective and was to some extent arbitrary. Except for those of Subjects 14 and 16, the great majority of false recognitions were couched in such terms as "Yes, I think so," "Think I have seen it," etc. These answers were separated from cases where the subject said, "No, I don't think so," or its equivalent, and were used exclusively as cases of false recognition. The high score of Subject 16 was largely due to her recognition of all but one of the white figures; and her definite answers as well as her over-recognition of white figures may have been due to a certain inattentiveness and carelessness when not constrained by instructions to attempt to see the white as a 'hole.' Subject 14, who also gave very definite answers, said that he used size rather than shape as a criterion for recognition. He made the poorest showing of any of the subjects; for although a couple of others had only three correct recognitions, they gave more than one doubtful answer to the right cards in the second series, and he was also one of the four (11, 12, 14, 18) who had more false than true recognitions. Of these four, Subject 12 presented peculiarities as well. In the first series he saw four of the six cases where instructions were to see a 'hole' in an ambiguous manner. His ability to see a 'hole' as compared with others was not markedly low, but the fluctuation of his attitude was great. In his case, no effect of ambiguity on recognition can be noted—two cases of recognition and two of non-recognition. One cause of non-recognition in some cases may be due to an often reported phenomenon, the shrinking in size of the memory image. Three subjects, 1, 2 and 5, said in one or two instances that they had not seen the figure before because this one was larger than the former. This may also account for the record of Subject 14. Eleven subjects made some use of images in recognition. A summary of their recognitions is given in Table IX.

Table IX.

No. of images produced by												
White figure, presented in						Hole, presented in						
Same way		Different way				Same way		Different way				Total all cases
R.	D.	R.	D.	N.	Total	R.	D.	R.	D.	N.	Total	
16	6	4	0	5	31	3	0	1	2	6	12	43

R. = recognized. N. = not recognized. D. = Doubtful.

The images do not appear to have aided recognition very much, but they do seem to have prevented false recognition to some extent. There are only 17 such false recognitions for 11 subjects as opposed to 30 for the other 9 subjects.

In conclusion, the interpretation of what the statistical results show is debatable. Considering the cases of recognition where the figure-ground relation was different in the two series, and the cases where least strain was present, we have no clear proof from this experiment—although certainly a suggestion—that figure dominance is important for recognition. Generally speaking, the experiment is not decisive either way. For instance, it might well be argued from Rubin's point of view that, where ambiguity was so easy, the cases we have cited above may not be genuine; that, in fact, the importance of an identity of figure-ground relation for recognition was here masked. The cards used by Rubin, and for this experiment, lend themselves to such doubt. For example, it is questionable whether a white figure seen as the 'mouth to a cave' really loses its figure character thereby. As Subject 20 remarked, "The cave mouth is more of an association than an expression of the fact that I am seeing through the black." The same subject also said that the white, however seen, remained the most important part of the perception.

That there was some difference of experience, however, when the subject reported 'hole' or white figure, is undoubted. But that this alteration of experience was ever a pure substitution of figure for ground character, or *vice versa*, or that it was the same change for the different subjects, is not at all established. And again, while we may be fairly confident that there was some difference of experience as the subject reported figure or 'hole' perception, we cannot be sure that there was no difference when he reported a figure perception or a 'hole' perception on both occasions. The types of perception for these cards do not seem to have been limited to two by our instruction, or, if the variation was one of degree, to have presented an 'either-or.' So long as this situation is unrectified, we cannot escape a lack of certainty in confirming or denying Rubin's interpretation. The use of the same cards in which the black was given a shiny surface, or the use of something like the formal patterns commonly stamped on linoleum, might facilitate a more rigid control of the alteration of experience. In any event (despite all the possible ambiguities), if the maintenance of a figure-ground relationship can be shown to be of prime importance in recognition, the results of this experiment suggest strongly that its *modus operandi* involves more than the repetition of an identical type of perception; that it demands the undisturbed re-excitation of a fairly well-established attitude. If the attitude is not stimulated by the objective conditions, and must be brought into being almost entirely by an effort at the behest of the experimenter's instructions, in so far it is an isolated phenomenon capable of repetition but not of the carry-over necessary for recognition. If the effort prevents the appearance of an ordinary reaction to the card presented or if the conflict with that reaction is too engrossing, recognition will be obstructed. In other words, the attitude toward the figure-ground complex must be or have become a more or less natural or habitual one, as was the case with the white figures.

But we have some reason to suppose (see Table VII) that where conflict and strain are avoided by facility in obeying directions, the maintenance of the same type of attitude in regard to figure and ground is of less importance for recognition. So, although it may be a well-formed attitude of this kind that is the cause of the conflict with instructions, and that produces strain, it does not necessarily follow that its presence is vital for recognition, or that its suppression, rather than the strain that it engenders, is responsible for the non-recognition. But, as we have shown, the strain does not produce non-recognition in itself, but works on something relating the two occasions of perception. What that is, though, we do not believe has been surely isolated in this experiment.

The experiment with the materials mentioned above should help in deciding how far the attitude expressed in the relation of figure to ground may be identified with this unknown something that is active in recognition. On the one hand, we wish to know whether a change of figure dominance is effective in obstructing recognition when strain is absent; and on the other, whether the figure dominance is incidental to the production of the strain or whether the strain affects recognition *via* an effect on figure dominance. For it might be that figure dominance was unimportant until it was brought into prominence through the strain occasioned by the conditions of the experiment—which amounts to saying it is of importance only for particular individuals who have settled tendencies of this kind, or for particular situations about which such settled attitudes tend to arise.

APPENDIX.

Ambiguities in first and second series.

	Change from																
	Hole to white figure								White figure to hole								
	According to instructions				Against instructions				According to instructions				Against instructions				
	R.	N.	D.	P.	R.	N.	D.	P.	R.	N.	D.	P.	R.	N.	D.	P.	Total
I	—	—	—	—	—	—	1	2nd	2	—	—	2nd	—	—	—	—	
First series	—	—	—	—	—	—	5	2nd	6	—	—	1st	—	—	—	—	
									10	—	—	2nd	—	—	—	—	
									18	—	—	2nd	—	—	—	—	
									18	—	—	1st	—	—	—	—	
									—	19	—	1st	—	—	—	—	
									—	12	—	1st	—	—	—	—	
									—	18	—	1st	—	—	—	—	
									—	18	—	1st	—	—	—	—	
Total for I	—	—	—	—	—	—	2	—	5	4	—	—	—	—	—	—	11
II Second series	—	12	—	1st	—	—	1	2nd	18	—	—	1st	3	—	—	2nd	
Total for II	—	1	—	—	—	—	1	—	1	—	—	—	1	—	—	—	4
III Unclassified	In first series: 19 "Half and half" recognized as white figure In second series: 5 Doubtful, hole required, white figure in first series 6 ,, ,, ,, ,, 14 ,, ,, ,, ,,																
Total for III	4 ...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	4
														Total		...	19

R. = recognized. N. = not recognized. D. = Doubtful. Numbers indicate subject.
P. = presentation.

(*Manuscript received* 27 *June*, 1932.)

A QUANTITATIVE VIEW OF PAIN.

By LEWIS F. RICHARDSON.

THE observations here recorded were made on the pain caused by cellulitis of the right thumb, which hurt me with varying intensities for seven weeks. The word 'pain' is used, following Wohlgemuth (1), for the sensation localized in the hand and distinct from the accompanying feeling of 'unpleasure.' Of course pain is really five-dimensional, the intensity being a function of one co-ordinate of time and three of space; but that view is too complicated for a beginning; and so, ignoring throbbings, let us specialize on pains that are constant in time, or nearly so. Further to simplify the scheme, it is possible to ignore the spatial distinction between a toothache and stomach-ache, and so to establish a one-dimensional sequence of pain-intensity, by studying the effects of pain in distracting from thought, and of thought in distracting from pain. Thus, in sequence of increasing intensity there were:

(A) a pain so slight that it was perceived only when carefully attended to,

(B) a pain which was obvious when attended to, but which ceased entirely when other matters were thought of,

(C) a severe pain which became more bearable when one engaged in lively conversation, or read an exciting story, or concentrated on a mathematical problem, or decided a difficult administrative question,

(D) a still more intense pain, which was little, if at all, diminished by the sort of distractions which were effective for (C). On some occasions the intensity of the pain (D) seemed to be somewhat increased by attempted distraction,

(E) a pain so intense that the rôles played in the process of distraction by pain and thought were interchanged; for the pain arrested the movement of thought, leaving a stationary mental state consisting of just oneself and pain. But this I experienced for only a few seconds at a time, during surgical operations. The phenomenon is vividly described by Stout (2).

There is another way of defining a 'critical intensity' of pain, here denoted by α, namely as that pain which just prevents one from going to sleep at the time and under the circumstances in which one does ordinarily fall asleep when free from pain. My observations showed that the intensity α was more than (C) and less than (D): sleep and distraction both became unattainable at about the same intensity of pain.

This observation accords with the theory that the physiology of falling asleep is similar to that of distraction. Important in that connection is Pavlov's work on sleep in dogs (3) and McDougall's discussion thereof (4).

As part of a series of researches into the quantitativeness of sensations, I attempted to assign numerical values to the typical intensities described above. The unit was defined so that α equalled 100. The pain (A) was produced by biting the uninjured left thumb. Then in the course of many days, it was estimated that (A) = 3, (B) = 20, (C) = 70 decreasing on distraction to 30, (D) = 200, (E) = 500. For other sensations it has been found that estimates by different people vary widely, and the same is to be expected for pain (5, 6).

To those who are accustomed to regard the process of measurement as being necessarily either (i) the marking and counting of units, or (ii) calculation using such counts (7), I should explain that there was no counting here nor any calculation. Instead ratios were estimated in a manner so direct as to elude analysis, and were expressed as numbers by means of past experience with centimetre scales. This intuitive directness, and the fact that so many people can estimate ratios of colour, loudness or other sensations, suggests that there may be in most mental experience a primaeval vague quantitativeness, out of which the refined conceptions of modern physical measurement have been gradually evolved.

There is a second critical intensity here denoted by β, namely the intensity of pain which spoilt one's control of voluntary muscles. As far as I can tell, β seemed to be about the same as the intensity (E) which arrested the movement of thought; for it was observed (i) that a pain of intensity about 70 to 150 continued for several days with but little ill effect on voluntary muscles, but (ii) a pain of intensity 300, which approached the intensity 500 at which pain prohibited thought, had after six hours' continuance begun to split the personality into a querulous child making uncontrolled movements *versus* an ashamed but helpless spectator, who had an awful sense of further disruption impending. But further disruption did not occur, for at this stage the physician gave me morphia.

The unpleasure accompanying the pain appeared, when intense, to consist of, or to be indistinguishably mingled with, visceral sensations. Wohlgemuth regarded them as distinct, but I did not separate them. Although the intensity of unpleasure rose and fell more or less with that of the pain, yet the changes of unpleasure sometimes lagged in time behind the changes of pain. For example, on having gauze drainers pulled out from surgical cuts, the pain ran up suddenly to intensity (E), thought stopped for a second or two, meanwhile intense unpleasure came on as a feeling of being all mixed up inside the torso. Then in the course of the next hour or so the pain decreased rapidly to (D) or (C), while the unpleasure decreased much more slowly; so that there was a stage at which the unpleasure was more conspicuous than the pain. But when on the contrary the pain had been nearly steady all day at intensities (C) or (D), then the unpleasure was less conspicuous than the pain; and it was the pain not the unpleasure that kept me awake. The feeling 'all mixed up inside' is reported in connection with unpleasure and fear by one of Conklin and Dimmick's observers (8).

Food had an astonishingly large effect on the pain. To the well-fed patient the presence of food in the stomach was of much less intellectual interest than the story which he was reading, yet breakfast regularly decreased the pain from 70 to 30, a decrease as great as could be achieved by close attention to the story.

The introspections recorded in this paper were made at times when the observer was free from the influence of any drug such as morphia, chloroform or nembutol,

In conclusion I formulate some questions for other observers, having already given my own answers above.

(i) Is $C < \alpha < D$?

(ii) Is $\beta = E$?

(iii) What is the ratio α/A?

(iv) What is the ratio β/α?

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(*Manuscript received* 18 *October*, 1932.)

NOTE ON DR WILLIAM BROWN'S PAPER ON A CENTRAL INTELLECTIVE FACTOR.

By GODFREY H. THOMSON.

(*University of Edinburgh.*)

In the October, 1932 number of this *Journal* my friend Dr Brown describes an experimental inquiry into the correlation of abilities, to the results of which he has applied Prof. Spearman's tetrad criterion. This work is of very great interest and all will look forward to the publication of the full mathematical analysis of the results which is promised.

But is it justifiable to say that the material "will furnish a precise and definite solution of the problem of a central intellective factor"? Certainly the calculations show so far a good correspondence between the actual distribution of tetrad-differences, and the theoretical distribution deduced from the theory that the sole cause of the correlations is a central component, active in every test. And it seems highly probable that further calculation will confirm this, except perhaps in regard to the exact form of the distribution. But the question of the cause of the correlations will still be quite undecided.

On no theory at all, by the laws of probability, there will be a tendency towards zero tetrad-differences among correlation coefficients. This tendency is the stronger the more complex the background of causes concerned in the correlations; and the mind is surely complex. When in addition the tests are selected "after much preliminary trial," and later it is "found necessary to reject one of the tests" (*i.e.* to reject 2907 tetrad-differences) and then further to reject "one of the correlation coefficients" which remain (*i.e.* to reject 272 more of the tetrad-differences), it would indeed be surprising if the surviving data did not show a very strong tendency to zero tetrad-differences. Nor can I for the life of me see what is then proved.

(*Manuscript received* 16 *November*, 1932.)

A NEW FORM OF TACHISTOSCOPE.

BY G. C. GRINDLEY.

(From the Cambridge Psychological Laboratory.)

IN many tachistoscopic experiments it is desirable that the instrument should be one in which (*a*) the figure to be exposed can be drawn on a card, without the necessity of making a lantern slide, (*b*) the whole of the figure appears together at the beginning of the exposure and disappears together at the end, without any visible movement of a shutter across the figure, (*c*) the observer's eye is correctly focussed when the figure appears, (*d*) the brightness of the pre-exposure field can be made equal to that of the ground upon which the figure appears, and (*e*) the length of the exposure can be varied over a fairly wide range. The following is a description of an easily constructed tachistoscope which satisfies these requirements.

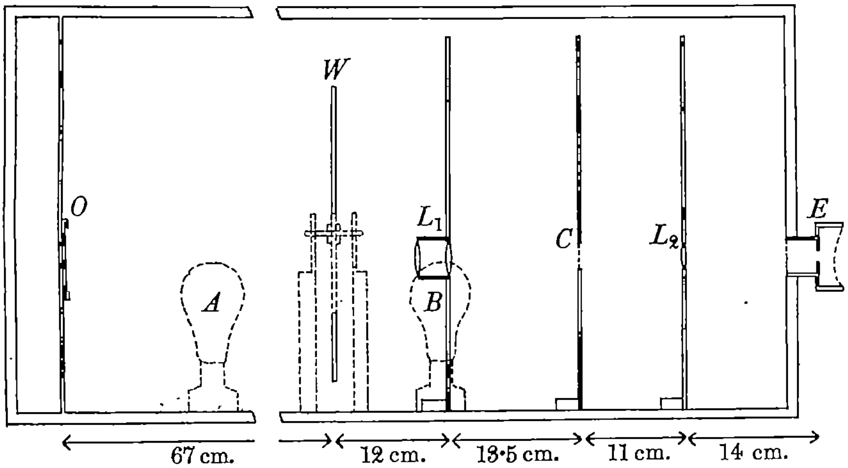

Fig. 1.

Fig. 1 is a diagrammatic vertical section of the instrument. The figure to be exposed is drawn on a card which is placed in a holder O. A camera lens L_1 forms an image of the figure in the plane of the cross-wires at C; and the observer looks at this through an eye-piece E and a lens L_2, which acts as a magnifying glass.

W is a wheel which is weighted on one side so that it will swing backwards and forwards like a pendulum. A window cut in the wheel exposes the figure for a short time during the swing. The front of the wheel is white, and two electric lamps *A* and *B* are placed so that they illuminate equally the card carrying the figure and the front of the wheel. (L_1, *C*, L_2 and *E* are shielded from the direct light of these lamps.)

The wheel *W* is placed at such a distance from the lens L_1 that the two lenses L_1 and L_2, acting together, form a real image of the wheel in the plane of the *lens* of the observer's eye. The observer, therefore, does not see the movement of the edges of the window even if the wheel is moved very slowly; but the whole of the figure appears gradually at the beginning of the exposure and disappears gradually at the end—as though the exposure were due to the opening and closing of a shutter in the lens of the observer's eye. If the wheel is allowed to swing fairly fast, the whole figure appears and disappears suddenly. Before the exposure the observer sees the cross-wires against a white background; and during the exposure the figure drawn on the card appears for a short time and then vanishes, with no apparent change in the white background (owing to the equal illumination of the wheel and the card), and with no visible movement of a shutter. If the observer's eye is focussed on the cross-wires before the exposure, the figure will be in focus when it appears.

The length of the exposure may be varied by having a window of variable size, or by varying the weighting of the wheel, or by varying the position from which the wheel is released, or by a combination of these methods. The time of exposure can be calculated roughly from the period of oscillation of the wheel, or it can be measured by means of a moving-plate camera. The wheel can be released and caught by hand, or, better, by mechanical catches. Unless great accuracy in the time of exposure is required, the construction of the wheel and its bearings can be very simple; for most purposes a wheel made from *Meccano* and covered with white cardboard is quite satisfactory.

It is convenient to place the whole apparatus in a large box or cupboard, as in Fig. 1, with the eyepiece projecting at one end and with openings in one side to allow access to the card-holder and the wheel. The lenses, the cross-wire, the wheel, and the card-holder may be mounted on separate wooden stands which can be fixed when their correct positions have been found. The lens L_2 and the cross-wires *C* are first placed at convenient distances from *E*, the distance between L_2 and *C* being slightly less than the focal length of L_2. The holder *O*, carrying a card with a figure, and the lens L_1 are then placed in such a position as to give a suitable magnification of the figure when it is observed through E, and to produce no apparent parallax between the figure and the cross-wires. The correct position for the wheel can then be found from the fact that if the wheel is rotated very slowly the apparent movement of the edges of the window will be in the same direction as the rotation if the wheel is too near to L_1, and in the opposite direction if the wheel is too near to O.

In the instrument in use at the Cambridge Psychological Laboratory the lens L_1 is a camera lens of 13 cm. focal length and 2·0 cm. aperture; while the lens L_2 is of 13 cm. focal length and 2·5 cm. aperture. The distances between the various parts of the apparatus are shown in Fig. 1, and with this arrangement a figure whose greatest length is about three inches appears to subtend an angle of about 7° at the observer's eye.

My acknowledgments are due to the Medical Research Council for a grant which made this work possible.

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PUBLICATIONS RECENTLY RECEIVED.

Nouveau Traité de Psychologie. Par GEORGES DUMAS. Tome Deuxième. Paris: Félix Alcan. 1932. Pp. 612.

The second volume of this comprehensive "new treatise" edited by Georges Dumas and others, is entitled *Les Fondements de la Vie Mentale.* It contains four parts. The first deals with *Les Réactions Élémentaires* and is mainly concerned with the physiological basis of sensation and emotion; the second with *Les Sensations,* and describes the main phenomena of the special senses; the third with *Les États Affectifs,* and discusses the disagreeable and the agreeable, pleasure and pain, emotional stimuli, emotions as experience, needs and instinctive tendencies; the fourth with *Les Images.* Practically all the work is on a high level, and it should certainly find a place in every psychological library. A large number of well-known authors have collaborated in the production of the volume and have taken their job very seriously. Good bibliographies are given. The whole work merits a very high place among existing text-books of psychology.

The Vibrato. Ed. by C. E. SEASHORE. University of Iowa Studies in the Psychology of Music, vol. I. Iowa: University Press. 1932. Pp. 382. $2.00 (paper bound), $2.50 (cloth bound).

This volume is the first of a new series of publications from the psychological department of Iowa University. Further numbers, devoted solely to the psychology of music, will follow every year or so; they are open to writers from other laboratories, and will include reviews of relevant literature.

The present number contains an introduction by Seashore, an exhaustive survey of work on the vibrato, and fourteen articles by Seashore's students. Psychologically the most valuable contribution of the laboratory would seem to be the apparatus for recording simultaneously the pitch, intensity, duration and timbre of sequences of musical notes. An ingenious adaptation of the stroboscope is used for photographing pitch to the nearest one-tenth of a tone; and by means of an oscillograph intensity is recorded directly in decibels. There are numerous illustrations of these records, as obtained from student singers and instrumental players, and from gramophone records of well-known artists.

The almost universal appearance of the vibrato among trained vocalists and violinists is established by these researches. Though considerable variations occur in rate and extent, even on successive notes in the same song, yet the average pitch vibrato ranges over approximately one semitone (half a semitone among violinists), at an average rate of 6½ vibrations a second. Similar fluctuations are found in intensity and timbre. The vibrato is less frequent among untrained singers, and very rare in pre-adolescent children. It is also shown that the normal ear is quite incapable of recognizing the wide extent of the oscillations; a pitch which wavers regularly half a semitone up and down is heard as practically constant. This accounts for much of the confusion that has arisen in discussions by musicians of the ethics of the vibrato. The far more noticeable and objectionable tremolo differs from the vibrato in its slower rate and greater extent, and in its irregularity.

As a contribution to aesthetics and musical pedagogy, the book is of much more doubtful value. The reviewer can find in it no evidence to substantiate Seashore's claim, namely that his phonophotographic methods constitute an objective approach to the study of the beautiful, or of emotional expression, in the performance of vocal music.

Bentham's Theory of Fictions. By C. K. OGDEN. London; Kegan Paul, Trench, Trubner and Co., Ltd. 1932. Pp. clii + 161. 12*s.* 6*d.* net.

Jeremy Bentham's views on 'fictions' or 'word magic' are collected in this book from various parts of his works, arranged systematically, and preceded by a detailed exposition by Mr Ogden. It is shown, in this introduction, that Bentham, although he has been generally neglected during the past century, very largely forestalled the *As if* philosophy of Vaihinger, and Ogden and Richards' investigations of *The Meaning of Meaning.* Briefly stated, Bentham divided words into those that represent 'real entities' or physical objects, and those that represent 'fictitious entities.' The latter are mere names and verbalisms which have become stereotyped and reified; thus they are responsible for much confusion in every-day language, in philosophy, psychology, law, politics and religion. Through his elaborate analysis of the various orders and types of fictions the author attempted, single-handed, to build up a new system of logic, which should form a truly scientific basis for future investigation in every field of thought. The book (or at least Ogden's introduction to the book) should certainly be read by all who are interested in language and its influence on human thought and behaviour.

Mencius on the Mind. By I. A. RICHARDS. International Library of Psychology, Philosophy and Scientific Method. London: Kegan Paul, Trench, Trubner, and Co., Ltd. 1932. 10*s.* 6*d.* net.

Dr Richards, in this book, uses the analysis of pieces of the text of Mencius, a Chinese philosopher of the fourth century B.C., as a means of introducing the discussion of a number of problems in the psychology of language, translation and reasoning. He raises a number of interesting and important points, but the book suffers from the attempt to deal with too much material in too limited a space. From his analysis of the text he is able to show that the psychology of the Chinese mind of the fourth century was radically different from that of the modern western type, lacking, almost entirely, a cognitive aspect. Perhaps the most important section of the book is that in which the author discusses what he calls "Multiple Definition." This is a technique for comparative studies which proceeds by separating the different meanings of words, and exhibiting each meaning as a member of a coherent system of meanings of related words. Thus, for instance, there are many words which are common to Oxford and Cambridge philosophers; but there is a coherent 'Cambridge' system of meanings for these words, and another coherent 'Oxford' system. Dr Richards very reasonably contends that it is only by employing the method of Multiple Definition that comparative studies of different systems of thought can effectively be made.

The Mind and its Body: The Foundations of Psychology. By CHARLES FOX. International Library of Psychology. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1931. Pp. xii + 316. 10*s.* 6*d.* net.

In this book Mr Fox discusses the mind-body problem in the light of recent discoveries in physics, physiology and psychology. Mr Fox attacks three "dogmas associated with nerve physiology"—that the reflex is the sole type of nervous action, that there is a one-to-one correspondence between brain events and mental events, and that the cerebral cortex is in some special sense the organ of conscious life. He also argues that there is no reason to assume an underlying physiological basis for mental images. The concepts of Temperament, Mental Energy and Instinct are critically examined, and after a brief discussion of certain philosophical doctrines, Mr Fox concludes that the mind-body relation is a unique relation, in which the mind controls the body, without there being a one-to-one correspondence between the mind and the brain. The whole argument of the book is supported by many references to recent literature.

A Sociological Analysis of Certain Types of Patriotism. By EARLE L. HUNTER. New York. 1932. Pp. 263. $2.50.

This essay was written and published "in partial fulfilment of the requirements for the Degree of Doctor of Philosophy in the Columbia University." It deserves a wide public, for it is an unusually interesting and stimulating study. It appears that, in America at least, the evidence indicates that loyalty is directed primarily to specific objects, and can be defined "only in terms of specific or directly appreciable values." Now country is not such a specific value and hence it is very desirable to drop the term 'patriotism.' It is proposed to use the term 'communalism' instead, where "communalism is taken to mean that the general interest of the community is a controlling principle to guide the selection and revaluation of those immediate objects to which loyalty may be given." A great amount of specific illustration is given and the author raises in clear-cut fashion a large number of problems which will be of great interest to any social psychologist.

Biologie de l'Invention. Par CHARLES NICOLLE. Paris: Félix Alcan. 1932. Pp. xv + 160. 15 fr.

This is really a rather tumultuous, but very stimulating little book about genius. It is written in a personal and epigrammatic style, and it is not always easy to follow the author's flights of thought. He says himself that "la conclusion la plus claire de mes réflexions est que la faculté de l'invention est un accident, une propriété hasardeuse, ni plus ni moins extraordinaire que tant d'autres." However, it appears that in its most striking forms invention is an accident that can occur in the case of men only. When a women tries vainly to imitate a man in this respect the results are disastrous. And any way free play to invention may become easily a most serious social menace. In spite of the author's remark that he is "a perfect donkey in philosophy," his argument is perhaps rather less concrete than could be desired. But many brilliant things are said, and most readers will alternately applaud and hiss this extremely clever book.

Reflex Activity of the Spinal Cord. By R. S. CREED, D. DENNY-BROWN, J. C. ECCLES, E. G. T. LIDDELL, and C. S. SHERRINGTON. Oxford: Oxford University Press. 1932. Pp. viii + 183. 10*s.* net.

The plan which has been followed in this book "is to introduce and describe *seriatim* a number of reflexes...which take expression in the skeletal musculature, predominantly in that of the limbs. The description and discussion given of them in each case are directed mainly towards analysis of the working of the reflex mechanism." The aim of the book is to analyse reflex responses in terms of motor units, and to infer from the analysis the nature and properties of excitation and inhibition in the central nervous system, rather than to discuss the biological significance of reflexes, or to exhibit them as items in animal behaviour. For this reason, and also because spinal reflexes only are discussed here, this book is of less direct psychological interest than *The Integrative Action of the Nervous System*, but it will be welcomed by the psychologist, as well as by the physiologist, as an authoritative discussion of the present position of the study of reflex mechanism.

Animal Motivation: Experimental Studies on the Albino Rat. By C. J. WARDEN. New York: Columbia University Press. 1931. Pp. xii + 502. 25*s.* net.

Prof. Warden and his collaborators have been engaged since 1924 in an attempt to measure the strength of 'drives' such as hunger, thirst and sex in rats, under various conditions. In most of their experiments they have used the Columbia Obstruction Method, in which a 'drive' is measured by the frequency with which a rat will cross an electric grid in order to reach an appropriate incentive object (*e.g.* food). The experimental technique has been very carefully developed, and it constitutes an important advance upon previous methods of study in this field. But it is difficult, for several obvious reasons, to regard the results of experiments of this kind as being, in any strict sense, measurements of the strength of 'drives.' It may be objected, for instance, that a 'drive' cannot be measured by observation of only one kind of behaviour, such as crossing an electric grid; or that an animal's susceptibility to electric shocks depends on its general physiological condition (as is suggested by the results on the hunger drive), so that the deterrent effect of a given electric shock is not constant under different conditions; or that an electric shock produces fear, so that Warden's objection to the Choice Method (that it involves the simultaneous operation of two drives) applies also to the Obstructive Method. But the force of these objections will depend on the exact meaning given to the word 'drive,' and it must be admitted that the meanings which most psychologists attach to words of that kind are not sufficiently definite to permit accurate measurement by any method. It is interesting to notice that, measured by this method, and taking each drive at its maximum strength, the maternal is the strongest of the drives studied, and that thirst, hunger, sex and exploration follow in the order given. The experiments on the influence of segregation and of internal secretions on the sex drive are an important contribution to the study of sexual behaviour.

Most of the work described has already been published in separate papers and monographs; and these are reproduced in the book, with omissions which reduce, but do not eliminate repetition. It is convenient to have this valuable material collected together in one volume.

Human Learning. By EDWARD L. THORNDIKE. The Century Psychology Series. London: The Century Co. 1931. Pp. 206.

The Fundamentals of Learning. By EDWARD L. THORNDIKE. New York: Bureau of Publications, Teachers' College, Columbia University. 1932. Pp. xvii + 638.

Reward and Punishment in Animal Learning. By EDWARD L. THORNDIKE. Comparative Psychology Monographs, vol. VIII, No. 4, Serial No. 39. Baltimore: The John Hopkins Press. 1932. Pp. 65. $1.25.

In these three volumes Prof. Thorndike describes and discusses an immense amount of experimental work which he has recently done in an attempt to solve some of the fundamental problems of learning. *Human Learning* gives a summary and discussion of most of the results in a compact and readable form, while the actual experiments on human subjects are dealt with much more fully in *The Fundamentals of Learning,* and those on animals in *Reward and Punishment in Animal Learning.*

The problem of learning, to Prof. Thorndike, is the problem of how it is that the strength of a connection changes; the strength of the connection between a particular situation and a particular response being defined as the probability that the situation will evoke the response. The problem of learning, regarded in this way, is one which lends itself readily to experimental attack, as these volumes abundantly demonstrate. It is impossible to summarize the results in a short review. Since the whole method of the experiments depends on measurement of the probability that a given situation or stimulus will evoke a given response, most of the experiments involve a large number of subjects, and most of them are of the kind in which the subject learns a series of words or geometrical figures, or attempts to draw a line of a given length, rather than of the kind in which a detailed study is made of the acquisition of a complex skill. Many of the experiments deal with the operation of the Laws of 'Use' and 'Effect,' and evidence is given that while, as might be expected from the 'Law of Effect,' a satisfying after-effect can be relied on to strengthen a connection, an annoying after-effect has no uniform and direct tendency to weaken a connection. Prof. Thorndike has little that is favourable to say of the conditioned reflex theory or the Gestalt theory of learning. The supporters of those two theories of learning can be relied upon to attack him—from opposite directions. But whether Prof. Thorndike's 'connectionist' point of view is good or bad, his experimental results are a very important contribution to the study of learning.

The Economy and Technique of Learning. By WILLIAM F. BOOK. Boston: D. C. Heath and Co. 1932. Pp. x + 534. $2.00 net.

Prof. Book has presented in this work a very careful analysis of the theoretical and practical aspects of learning, based on the researches of others and on his own well-known experiments. It is primarily devised as a text-book in educational psychology, and the emphasis is always placed on the educational rather than on the industrial aspects of the subject. Parts I–IV, which occupy three-fifths of the book, deal with the theoretical aspects—the when, the wherefore and the why of learning. But perhaps the most important portion is Part V, which is a discussion of the ways to make learning more economical and efficient. In this part the questions of motivation, fatigue, and transference are discussed at length, and due consideration is given to the arrangement of the objective conditions of the learning situation. If there is a defect it is that perhaps more could have been said about the part-whole problem, and about the effect of the distribution of practice periods—a different, though allied, question to that of rest pauses which is also dealt with. Some mention might have been made in the section on transference of the work of Skaggs and others on retroactive inhibition, in which it was shown that the greater the objective similarity between tasks the greater was the interference (negative transference). This appears to be important as evidence against the universal validity of the Thorndike theory of common elements, especially as somewhat similar results have been found in some of the transference experiments with card sorting.

Fluctuations in Human Output. By S. J. F. PHILPOTT. *British Journal of Psychology Monograph Supplement*, No. XVII. Cambridge University Press. 1932. Pp. viii + 125. 12*s.* 6*d.* net.

Dr Philpott presents in this study the results of his experiments on uninterrupted mental work. His experiments are divided into five series, the earliest dating from before the War and the last being a recent study on a large number of boys of school age. The tasks he used were dotting, various arithmetical tasks, sound discrimination, and an illusion of reversible perspective; and he endeavoured to find out whether there was any periodicity in the efficiency of performance. In the dotting test and in the different arithmetical tests a time signal was given every 5 sec., and the subjects began afresh immediately. Dr Philpott mentions the danger—also noticed by Flügel in *Practice, Fatigue and Oscillation*—of using an artificial time interval in experiments upon individual fluctuations of efficiency, namely, that the artificial interval itself may override an individual's natural tendencies, and develop an artificial periodicity. He points out on the other hand that, "As long as the work itself is discrete there will be danger of rhythm. The subject is as much in danger of swinging through his dots or sums in groups of one, two, three, or more at a time, as he is of swinging through time intervals in such groups." Dr Philpott's results showed that the periodicity in mental work appears to be of a geometrical nature. Furthermore, only a few such geometrical periods were found, and the curves of all individuals for the different tasks they were set appeared to belong to one or other of these periods.

Property. A Study in Social Psychology. By ERNEST BEAGLEHOLE. London: Allen and Unwin. 1932. Pp. 326. 10*s.* 6*d.* net.

Mr Beaglehole presents in this book a detailed study of property for the purpose of discovering whether there is an instinct of acquisition. In the case of insects and birds he thinks that acquisitiveness is in general only one link in a wider chain of behaviour. In primitive society also it does not exist as an instinct in the proper sense of the term, but it is merely associated with the particular sentiments that it is customary for individuals within that group to build up, and thus, unlike a true instinct, it is liable to modification according to changes in the particular sentiments which different groups tend to develop, and which the same group tends to develop at different times. Similarly, in the case of western civilization the same conclusions are reached. Such things as the mania for collecting postage stamps or birds' eggs, which appear to have lost considerable popularity during the last thirty years, Mr Beaglehole thinks are far more easily and satisfactorily explained in terms of the greater opportunities for using leisure to-day compared with thirty years ago, than in terms of the waxing and waning of a specific instinct. "It is the moulding of human nature by the culture patterns of the different groups which adapts one child to a communist organization of society and another to a western capitalist society." Finally it should be mentioned that not the least important part of Mr Beaglehole's book is a statistical analysis of property values among primitive races along the lines of the well-known investigations made by Hobhouse, Ginsberg and Wheeler into other social institutions.

Studies in Sociology. By MORRIS GINSBERG. London: Methuen. 1932. Pp. ix + 211. 6*s*. net.

This book consists of a collection of essays. The first five are predominantly sociological, and of these chapters I–III are on broad sociological theory, while chapters IV and V deal with the concept of evolution in sociology. Chapters VI and VII concern two of the most important psychological factors in sociology, namely instinct and emotion. In chapter VI the various uses of the term emotion are discussed, and suggestions are put forward concerning the various points that a theory of emotion needs to take into account. In chapter VII much the same thing is done with instinct, to show the real importance of the conception in spite of the many attacks that have been made on it recently in America. Chapters VIII, IX and X deal with biological factors in sociology, chapter IX containing a statistical analysis of the mobility—or rather the lack of mobility—between different classes, based on three separate enquiries undertaken by Prof. Ginsberg. Finally, chapter X, a hitherto unpublished paper, contains a review of the claims of eugenics, and points out where Prof. Ginsberg thinks that the eugenists have been over-confident, and along what lines he considers that their researches will be most fruitful.

Psychodiagnostik. Von HERMANN RORSCHACH. Bern und Berlin: Hans Huber. 1932. Zweite Auflage. S. 230. Sv. Fr. 18 ($3.60).

Rorschach's extremely interesting work dealing with the diagnostic significance of certain ink-blot interpretations is already well known in this country. This fine and well-printed edition should bring him many new readers and stimulate other investigators to test further his methods and conclusions. The present edition is in two volumes, the first of which describes the technique, results and conclusions of Rorschach's tests, and the second contains some finely reproduced figures of his particular series of ink-blots. It is vastly to be regretted that Dr Rorschach died while yet his work was at its beginning, for he was a man who had it in him to do a great amount of good work in psychology. This edition is prefaced by a brief account of Rorschach's life and work.

La Psychognomie: Lecture Méthodique et Pratique du Caractère et des Aptitudes. Par PAUL BOUTS et CAMILLE BOUTS. Paris: Félix Alcan. 1932. Pp. xv + 210. 40 fr.

This is a most astonishing book, most pleasantly printed and profusely illustrated, and devoted to a rather dogmatic recital and exposition of the great practical value of phrenology, physiognomy and graphology. No doubt many people may find it interesting and those who have little psychological training or aptitude may find it convincing. At least it is amusing.

Talents and Temperaments: By ANGUS MACRAE. Contemporary Library of Psychology. London: Nisbet and Co. 1932. Pp. xi + 210. 5*s*. net.

Dr Macrae is in charge of the Vocational Guidance Department at the National Institute of Industrial Psychology. He therefore had the most excellent opportunities to write with knowledge and judgment on this topic. He has used his opportunities well and has produced a book which is both a pleasure to read and an authoritative statement of the growth, the present position, the achievements and the possibilities of the vocational guidance movement. Dr Macrae shows how vocational guidance grew up, the contribution of the early work on the measurement of intelligence and the use of specific tests of ability being clearly summarized. He then shows by what method it is now prosecuted, how the 'standardized interview' and the appraisement of temperament and character come in. He indicates the great help that may be given by the judicious parent and the wise doctor. He describes experiments on vocational guidance which have been conducted, and suggests ways in which they need continuation and extension. He writes finally of the probable future of the movement. The whole book is the outcome of wide and careful reading and, more important still, of a mass of original research. It deserves a wide public.

Personality, Many in One: An Essay in Individual Psychology. By J. W. BRIDGES. Boston: The Stratford Company. 1932. Pp. vi + 215. $2.00.

Although this book is directed rather to the general reader than to the trained psychologist, the latter will find in it much that will hold his attention and stimulate his thought. Among this perhaps the most interesting sections are those that deal with ancient and recent psychologies of types and with the problems of temperament and character. The various different attempts that have been made to classify human individuals into types are well and clearly dealt with, and as the original references are provided in the text and in the bibliography, Dr Bridges' treatment is a very useful introduction to a most fascinating study. Temperament is treated as a group or organization of affective functions and character as a group or organization of active 'drives.' This distinction is perhaps a somewhat narrow one, but the resulting discussion is one of great interest. In fact the whole book succeeds in being clear about a lot of matters that are usually left in a mist, and as an introduction to its topic merits a high place.

Psychopathic Personalities. By EUGEN KAHN. Yale University Press and Oxford University Press. 1931. Pp. xii + 521. 22*s*. 6*d*. net.

In this work Dr Kahn discusses psychopathic, not abnormal, personalities, psychopathic personalities being those whose deviations from the normal, while not so gross as to be psychotic, are such that they either suffer themselves or cause suffering to society. Personality he considers to be made up of impulse, temperament and character and, as such, he shows how dependent normality is both on endowment and on environment, stressing the importance of the former, in contradistinction, for example, to Adler's teachings, and showing how psychopathic personalities are likely to arise when the endowment is either too liberal or too meagre. He also points out the importance in psychotherapy of the psychopath being educated to understand himself and his limitations. There is a bibliography of nineteen pages, almost wholly to German literature, and Dr Kahn discusses and criticizes the views of other writers with a restraint which gives much force to his own arguments. *Psychopathic Personalities* will be found to be very helpful, especially by those who have not kept in close touch with the voluminous German literature on this subject.

The Psycho-Analysis of Children. By MELANIE KLEIN. London: The Hogarth Press. 1932. Pp. 393. 18*s*.

This volume is No. 22 of the International Psycho-Analytical Library. The translation, by Alix Strachey, reads well, and is, on the whole, extraordinarily well done. The book itself will undoubtedly annoy some people and please others. Even those who are the least inclined to accept its theories will admit that it contains evidence as well as interpretation, argument as well as dogmatic assertion. It is in fact a very fine record of a mass of difficult and original research. Mrs Klein differs from Anna Freud in that she maintains that a method which is the equivalent of adult analysis can and should be used with children. She gives evidence to show that if it is so used transference-situations will develop in the regular way, and there need be no weakening of the child's 'super ego.' or its functions. The book itself is very largely case study and revolves mainly around the sexual impulses and anxiety and the profound disillusions of early childhood. To most ordinary people who read this book, and very likely to most ordinary psychologists also, it will appear to be a contribution to child pathology rather than to child psychology. But whichever future investigation may prove it to be, it is undoubtedly a pioneer work of great importance.

Psycho-Analysis of the Neuroses. By HELENE DEUTSCH. London: The Hogarth Press. 1932. Pp. 237. 10*s.* 6*d.*

This vol. is No. 23 of the International Psycho-Analytical Library. It consists of eleven lectures dealing with: (1) hysteria, (2) phobia, (3) obsessional neurosis and (4) melancholia. The argument is generally clear and always persuasive, and the book is packed with original clinical material. The general scheme is to illustrate as clearly as possible how psycho-analysis traces the origin of neurotic disease to: "(1) *fixation* of the libido, (2) *regression* and (3) the so-called current or '*actual*' *cause*, which may become through frustration a decisive factor in the genesis of the illness." The translation is by W. D. Robson-Scott.

Allgemeine Neurosenlehre auf psychoanalytischer Grundlage. Von HERMANN NUNBERG. Bern und Berlin: Hans Huber. 1932. Pp. 339. RM. 12.50.

This book contains a brief appreciative foreword by Prof. Freud.

It is a very capable study of the theoretical bearings of psycho-analysis in regard to the understanding and treatment of neurosis. The treatment is comprehensive and clear, and if it is not particularly original it has the great merits of being undeniably well informed and systematically organized. Practically all the major problems of the classification and psychological bases of the neuroses are discussed, in a general sense, and a final chapter deals with therapeutic methods. A useful selected bibliography is appended, together with an excellent index.

Intelligence and Mental Growth. By CLAUDE A. CLAREMONT. Psyche Miniatures. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1927. Pp. 138. 2*s.* 6*d.* net.

An apology is due to Mr Claremont for this very belated notice of his exceedingly interesting and attractive essay. The problem with which he is mainly concerned is the very ancient one of how best to describe or characterize intelligence. Briefly his answer is that intelligence consists in a reaction to causal relationships, or, more accurately, to *necessary* relationships, with a realization of their necessary character. The view is abundantly illustrated, very often in a highly original manner, and is argued most persuasively and consistently. In the course of his discussion Mr Claremont makes some acute and critical remarks about discovery and invention, intelligence tests, the 'general factor,' the creative element in thinking, and about instinct and intelligence. Perhaps the one obvious point of criticism is that 'causal relationship' is used in an extremely wide and somewhat indefinite way. But this is not a very serious point, for Mr Claremont is well aware of the difficulty and does his best to remove it. In fact the essay is an original effort, well written, suggestive and original in its ideas, and worthy of a careful and considered study.

An Analysis of Certain Psychological Tests by the Spearman Factor Method. By C. Jorgensen. London: E. A. Gold and Co. 1932. Pp. 70. Price 3*s.*

Do psychological tests measure what they are supposed to measure? To what extent is general intelligence a factor in each? What degrees of reliability do the Spearman methods give in actual use? These are some of the questions to which the author set out to try to find answers by giving a number of tests of general and special abilities to college students and analysing the results by the Spearman methods.

Owing to certain unavoidable shortcomings in the technique employed the findings are more suggestive than conclusive. Among the most interesting or challenging may be mentioned—(1) Many so-called intelligence tests are of little value, for they contain little that is general and much that is specific. (2) Tests of special abilities composed of a number of sub-tests are not likely to be of much value owing to the difficulty of interpreting an obtained score. (3) In respect to intelligence tests, doubt is thrown on such widely accepted beliefs as the efficiency of 'equally familiar' or 'entirely unfamiliar' material, and the superiority of long tests. The conclusion is reached that psychological tests should be handled and interpreted with extreme caution. The Spearman methods used gave evidence of satisfactory reliability under certain conditions, but lack of such reliability under other conditions was indicated.

The Dynamics of Education. By Hilda Taba. London: International Library of Psychology, etc. Kegan Paul, Trench, Trubner and Co., Ltd. 1932. Pp. xvi + 278. 10*s.* 6*d.* net.

This is a rather general essay mainly devoted to exposition of the view that education has to fit students to a world of constant change and progress, and that the change is on the whole of a definitely purposive order. The methods of education must be adjusted to these facts. Many current views are discussed in a clear and persuasive fashion. Much space is devoted to the psychology of learning. As giving a kind of general orientation the volume will be found very useful, particularly by those already in sympathy with its point of view. An appreciative foreword is contributed by Prof. W. H. Kilpatrick.

Defects of Speech in School Children. By H. T. Parker. Melbourne: Melbourne University Press. 1932. Pp. 96. 4*s.* net.

This is a report of work made possible and published by the Australian Council for Educational Research. It consists of a systematic study of various speech defects in normal and subnormal children in the schools of Tasmania. Speech defects are found to be more common than is usually suspected. They are classified under the heads of stammering, organic disabilities, lisping, baby-talk, and whispering. Sex differences are reported, there being a preponderance of speech defects in boys in all the cases except that of whispering. Subnormal children show far more speech defects than the normal. Remedial methods are discussed and evaluated.

Australian Educational Studies (First Series). Melbourne: Melbourne University Press. 1932. Pp. 125. 5*s.*

This publication contains the following papers: *The Length of the Teacher's Professional Life,* by K. S. Cunningham; *An Experiment in the Teaching of Reading Comprehension,* by G. Limb and H. T. Parker; *Standardized Tests of Teaching Ability,* by P. R. Cole and R. K. Whately; *Problem Children in Melbourne Schools,* by K. S. Cunningham; *Crippled Children in Tasmania,* by H. T. Parker; *The Story of an Australian Nursery School,* by M. V. Gutteridge.

Class Grouping in the Primary School. By H. S. WYNDHAM. Melbourne: Melbourne University Press (Macmillan and Co., Ltd.). 1932. Pp. 139. 6*s.*

This is a survey of methods of class grouping in the schools of Sydney. It is carefully carried out and the results are expressed in a clear and straightforward manner. The recommendations include the provision of more special schools, for defective, dull, and very bright scholars; the use of mental age as the major determinant of class-grouping of the central group of the school population; a greater fluidity of shifting from class-group to class-group, and improved methods of testing. The survey is of considerable interest to any administrative service which is concerned with school organization. The monograph is published by the Australian Council for Educational Research.

The Case for Curriculum Revision. By G. S. BROWNE. Melbourne: Melbourne University Press (Macmillan and Co., Ltd.). 1932. Pp. 183. 4*s.*

Although this monograph, which is issued by the Australian Council for Educational Research, has special reference to the revision of curricula in the public schools of Victoria, it is the outcome of wide experience and observation in Great Britain and America. The keynote of the proposals is found in the statement: "The old curriculum was tested in terms of power *to learn*: the new curriculum should include this and add a test in the power *to do.*" A great many specific proposals are made and a number of 'specimen courses' included. The report is written with enthusiasm and insight and should attract general attention.

The Joy of Life. By HERBERT N. CASSON. London: The Efficiency Magazine. 1932. Pp. 145. 3*s.* 6*d.*

This is the author's eightieth book. It states, with a great many illustrations and in a dogmatic and crisp manner, how the writer thinks that joy, and therewith success, in life may be achieved. His recipe is: "Just as light is composed of the five simple colours, so the Joy of Life is created by the blending of the six normal activities of human beings—doing, getting, helping, learning, playing and loving." The book contains masses of good advice and may be found stimulating by many readers.

Symbolic Distance. By S. BUCHANAN. Psyche Miniatures. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1932. Pp. 110. 2*s.* 6*d.* net.

Interpretation and Analysis. By JOHN WISDOM. Psyche Miniatures. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1931. Pp. 136. 2*s.* 6*d.* net.

The Nature and Mechanics of Consciousness. By MAX MUNDLAK. London: John Bale, Sons and Danielsson, Ltd. 1932. Pp. 246. 10*s.* 6*d.* net.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

GENERAL MEETINGS.

December 10, 1932.	Annual General Meeting. "Methods of Occupation Analysis in Relation to Hospital Nursing: a Preliminary Study." By Miss A. A. Frankford. "An Experimental Investigation of Non-Perceptual Structures." By Miss M. R. Harrower. Demonstration: "A two-story maze with records of left-hand reversal from trained and untrained subjects."

Lectures.

January 13, 1933.	"The Sense of Beauty." By Prof. S. Alexander.

SECTIONAL MEETINGS.

Medical.

December 14, 1932.	Symposium: "Psychology and Psychical Research." John Rickman, Mrs E. Brackenbury, William Brown, Edward Glover.
January 25, 1933.	Address from the Chair: "The Science of Error." By Dr E. Graham Howe.

Education.

December 8, 1932.	"The Teaching of Handwriting." By Miss Marion Richardson.
January 4, 1933.	"The Work of the University of London Institute of Education." By Prof. H. R. Hamley.
February 6, 1933.	"The Contribution of the Psychologist to the School." By Miss N. M. Tydeman, B.Sc.

Industrial.

December 8, 1932.	"The Relation of Laboratory to Field Investigations in Industrial Psychology." By Prof. F. C. Bartlett, F.R.S.
January 26, 1933.	"Psychology and Theatrical Production: a Plea for Rationalisation." By Mr Oliver Raphael. "Some Fields for Research in the Theatre." By Mr Russell Sedgwick.

Aesthetics.

December 9, 1932.	"Form and Representation in Pictures." By Mrs Helen Knight.
January 20, 1933.	"Aesthetic and Artistic Experience." By Miss P. Govan, B.A.

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